MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

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Dr. H. KNOX-SHAW, PRESIDENT, in the Chair.

The election by the Council of the following Fellows was duly confirmed :—

Clifford Chaffer, M.A., Rusland, Somerset Way, Iver, Bucks ;
Donald Percy Dalzell, M.A., 28 Albany Park Road, Kingston-upon-Thames ;
Alan Fletcher, M.A., 66 Mount Street, Lincoln ;
Alfred Henry Hockey, 86 Locket Road, Wealdstone, Harrow ;
Abbé G. Lemaître, Sc.D., Ph.D., Professor of Mathematics, Louvain University, 40 rue de Namur, Louvain, Belgium ;
Wyndham Edward Buckley Lloyd, M.A., Bryn Cadwrfa, Montgomery ; and
Richard Alexander Robb, M.A., B.S., M.Sc., F.R.S.E., Lecturer in Mathematics, University of Glasgow.

The following candidates were proposed for election as Fellows of the Society, the names of the proposers from personal knowledge being appended :—

Robert H. Baker, Ph.D., Professor of Astronomy and Director of the Observatory of the University of Illinois, Urbana, Ill., U.S.A. (proposed by P. Fox) ;
Rev. John P. Delaney, S.J., A.B., A.M., Professor of Physics and Seismology, Canisius College, Buffalo, N.Y., U.S.A. (proposed by P. A. McNally) ;
C. T. Elvey, Ph.D., Yerkes Observatory, Williams Bay, Wis., U.S.A. (proposed by O. Struve) ;

Rudolph Moritz, B.Sc., K.C., 4 Brick Court, London, E.C. 4 (proposed by F. J. Hargreaves); and
Rev. Gilbert Lord Wilson, Ph.B., 342 N. Madison Avenue, Pasadena, Cal., U.S.A. (proposed by F. Ellerman).

Seventy-two presents were announced as having been received since the last ordinary meeting, including:—

Helsingfors Observatory: Catalogue Photographique du Ciel, Tome viii. fasc. 1.
W. de Sitter: The Expanding Universe.
P. Stroobant: Les Observatoires astronomiques et les Astronomes, 1931.

Professor W. de Sitter delivered the George Darwin Lecture, taking as his subject "Jupiter's Galilean Satellites."

Jupiter's Galilean Satellites. (George Darwin Lecture, delivered by Professor W. de Sitter, Assoc.R.A.S., on 1931 May 8.)

I.

The four previous George Darwin lecturers have apologized for taking a subject which was not connected with the lifework of the man whose illustrious name these lectures are meant to commemorate. I am more fortunate, for, although Darwin never worked on the theory of the satellites, his researches on tidal friction and cosmogony often brought him in contact with it, and I have accordingly in several points derived great advantage from the study of his works. In a remarkable series of splendid papers closely following each other in the course of less than three years, and of which the last was written just fifty years ago, he repeatedly had occasion to consider the system of Jupiter and its four large satellites.

The satellites of Jupiter have, ever since their first discovery by Galileo on 1610 January 7, played an important part in the development of astronomy. For Galileo himself and many of his contemporaries they were a weighty argument in the strife between the rival systems of Copernicus and Ptolemæus. Soon afterwards, on the occasion of the foundation of the Paris Observatory (1669), the fame which he had achieved by making the first good tables of the satellites led to the choice of Dominico Cassini as its first director, who thus became the founder of a dynasty of astronomers, which was only dethroned by the French Revolution (1793). In 1676 Römer discovered the finite velocity of light from observations of the first satellite. Throughout the eighteenth century the theory of the satellites formed the subject

of the interest and the researches of the greatest astronomers. In England the great Bradley observed them at Wanstead, and discovered, probably about 1735, the " great inequality " in the motion of the second satellite, of which the period (in the eclipses) " nearly answers to the time the three inferior satellites take in returning to the same situation with respect to each other, and to the axis of the shadow of Jupiter, which is 437 days, or after 123 revolutions of the second satellite." Bradley further remarks: " It is manifest this difference did not arise wholly from the eccentricity of the orbit, if it have any ; but to what cause it is owing we are hitherto ignorant. In the mean while, till we can get more light in this matter from future observations, it were to be wished that some geometer, in imitation of the great Newton, would apply himself to the investigating these irregularities, from the certain and demonstrative principles of gravity."

Pehr Wargentin published his first tables of the satellites in 1746, in which the great inequality of 437·6 days, independently discovered by himself, was included for the Satellites I. and II. He ascribed it to the mutual gravitational action of the satellites, without, however, giving any analytical derivation. He made and published improved editions of his tables from time to time until his death in 1783. Evidently he was an expert in the handling of great masses of figures (he is also to be reckoned amongst the pioneers of scientific statistics) and the discussion of observations, and his tables, though purely empirical, represent the observations on the whole quite as well as those of Delambre, based on theory and on a further half-century of observations.

During the last third of the seventeenth and the whole of the eighteenth century the eclipses of the satellites were diligently observed. These observations were at that date the principal means for the determination of longitude, and the great observatories, especially Paris, joined about 1680 by Greenwich, observed the eclipses as frequently as possible to provide corresponding observations. Practically no progress was made with the mathematical theory until it was proposed by the Paris Academy as the subject of a competition, the prize to be adjudged in 1766. The essay by Lagrange, which won the prize, may be considered as the firm foundation on which not only the theory of the satellites, but the greater part of celestial mechanics during the next hundred years has been built.

From the time of the first Cassini the observations of the satellites had been practically restricted to the eclipses. Delambre, Laplace, and others had pointed out the desirability of observing also the occultations, transits and shadow transits, but no serious attempt was made to do this until Dr. Innes started his campaign of observations in 1908, to which reference will have to be made further on.

Observations of the satellites not in superior or inferior conjunction with the sun or the earth had, with the exception of those made by the discoverers, Galileo and Marius (Simon Mayer), and some of their contemporaries, only been made with the special purpose of determining the mass of Jupiter, until Sir David Gill, assisted by Mr. Finlay,

began his splendid series of heliometer observations in the autumn of 1891. In these observations the mutual distances and position angles of the satellites were determined, each of the six possible combinations of the four satellites in pairs being observed on every night, whenever possible. Gill's unsurpassed qualities as an observer were then at their height, and these observations are probably the best he ever made. The probable error of one observed distance or position angle is only $\pm 0''{\cdot}075$,* and although of course photographic observations with long focus telescopes reach a much higher precision, this series has by far the greatest weight of any single series of observations of the satellites.

Simultaneously a series of photographic observations was begun at Helsingfors by Donner and at Pulkowa by Kostinsky with Carte du Ciel instruments, both inspired by Backlund. These observations, which were continued till 1898, are not comparable in accuracy with Gill's heliometer observations, nor with the later photographic plates taken at the Cape, Greenwich, Leiden, and the Yale Southern Station at Johannesburg. Thus we can date the commencement of modern observing of the satellites from Gill's work in 1891.

It is a feature of Gill's observations that the satellites are only compared with each other, and not with the planet. Thus large systematic errors are avoided. Bessel's heliometer observations, in which the satellites were referred to both rims of the planet, are affected by very large systematic errors, which cannot be eliminated by the subsequent reduction or discussion. The same applies to measures on photographic plates. On the plates from Helsingfors and Pulkowa mentioned above Renz measured the satellites with reference to the planet, and it can be shown that the resulting differences of R.A. and decl. contain very large systematic errors.† In this case the planet can be rigorously eliminated as the separate pointings have been published, and the relative positions of the satellites can be derived from these without any reference to the planet. The same cannot be done in the case of the photographs taken at the Cape in 1902 and measured by Cookson. These have therefore been rejected.‡ The same method of observing the satellites relatively to each other had been applied some years previously by H. Struve in observing the satellites of Saturn with the micrometer at Pulkowa, concurrently, however, with measures referred to Saturn, or to the ring.

The observations of eclipses are determinations of time, and their purely accidental errors are small—of the order of a few seconds.

* The probable errors are :

Gill, distances	$\pm$ 0″·068
pos. angles	$\pm$ ·080
Finlay, distances	$\pm$ ·080
pos. angles	$\pm$ ·104
Gill and Finlay, distance and pos. angle	$\pm$ ·073

† *Groningen Publications*, **17**, pp. 50–52, 1907.

‡ *Proceedings Roy. Acad. Sci. Amsterdam*, **10**, p. 661, 1908. This series of plates has, however, been used for the latitudes, where the elimination of the pointing on the planet can be effected with more certainty.

Comparing the accuracy to be derived from eclipse observations and from the Cape heliometer observations of 1891, I came to the following conclusion (1902): "As a general, though necessarily somewhat vague, conclusion we can say that for the determination of those unknowns which depend on longitudes and radii vectores, the ancient eclipses are very far inferior to the heliometer, and the modern (visual) eclipse observations can only give about the same accuracy if a very large number of eclipses is observed under such favourable conditions as will never be realised in practice."

This conclusion was based on the consideration of the differences of the observations of the same phenomenon by different observers. It has long been known that the ordinary eclipse observations, *i.e.* the observation of the time when the light of the satellite disappears or is first seen again, are subject to large systematic errors, but it was thought that these were mostly of personal or instrumental origin, or due to circumstances connected with the observation, such as the transparency of the atmosphere, the presence of moonlight, etc. It was hoped that they could be eliminated by adopting other methods of observing, in which the phenomenon observed was sharply defined. Two attempts were made in this direction, viz. the photometric observations of the eclipses at the Harvard Observatory from 1878 to 1903, and the observations by Dr. Innes at Johannesburg with a high power, so that the discs of the satellites are seen and the progress of the phenomenon is watched in detail. At the time when I came to the conclusion mentioned above, the Harvard eclipses had not been published, and Innes's observations had not even been planned. The above quotation accordingly goes on: "The photometric observations can reasonably be expected to give a higher precision than the heliometer."

These hopes have, however, been disappointed. Both the photometric observations by the Harvard Observatory and the disc observations by Innes have been shown to be affected by systematic errors of the same order as those found in ordinary observations of the last and first speck of light. It is also a significant fact, shown by the discussion of ancient observations of eclipses, that the accuracy does practically not increase with the improvement of the telescope. Further, it is found that often observations of the same eclipse by different observers at widely separated places show the same errors. These errors probably are not in the observation, but in the observed phenomenon, due to refraction and absorption of the sun's rays in Jupiter's atmosphere. I have by these considerations been led to the conclusion that observations of eclipses, however carefully made, cannot determine the time with a greater accuracy than $\pm$ 10^s, and this limit cannot be lowered by combining a great number of observations. A very similar opinion had been expressed by Bradley in 1728.*

The principal aim of Gill's series of observations in 1891, beside the determination of the mass of Jupiter, was that of the positions of the orbital planes of the satellites. From eclipse observations these

* *Miscellaneous Works and Correspondence*, p. 12.

are derived from the variation of the duration of the eclipses, and although this determination is a fairly good one so far as the position of the node is concerned, the inclinations are on the one hand not separable from the ellipticity of the planet, and on the other hand are so strongly dependent on the systematic errors that their determination is very uncertain. As Bradley remarks : " It is certainly a very difficult matter to determine accurately the situation of circles so extremely small, nor is it to be undertaken without exquisite telescopes."

Also the importance of the determination of the inclinations was strongly urged on Gill by Adams before his (Gill's) departure for the Cape, and it was one of the principal aims of his great series of heliometer observations. At the present time they are the best known of all the elements.

As to the mass of Jupiter, the determination of this constant from observations of the satellites requires a very accurate knowledge of the scale value. At the epoch of Gill's heliometer observations this was provided by the standard stars which had been triangulated for the determination of the solar parallax from minor planets, in 1889-90; the distances between pairs of stars in this system were known to 1 part in 180,000 by a combination of the results of many observatories. This standard distance was transferred by the heliometer to a pair of stars in the neighbourhood of the satellites (and approximately in the position angle of the line of their motions), which consequently became known to within 1/100,000th of its amount. The uncertainty added in the determination of the mass by the accidental errors of the observations of the satellites is insignificant. The adopted reciprocal of the mass of Jupiter, which is a weighted mean from these observations, those of Cookson in 1901 and 1902, and the determinations from perturbations of planets by Newcomb, Hill, and Krüger, is $1047{\cdot}40 \pm 0{\cdot}03$, and it seems certain that the probable error given is a reliable measure of the accuracy obtained. The mass is thus known to 1/33,000th of its amount, *i.e.* better than any other mass in the solar system.

It has sometimes been suggested that a still better value of the mass might be derived from the outer satellites, the eighth or the ninth. The elongation distance of each of these satellites from Jupiter is about two degrees, and the uncertainty of the computed value of this distance owing to the uncertainty of the mass is only $\pm\ 0''{\cdot}07$. Thus, in order to make a determination of the mass from one of these satellites worth while, it is required that this distance of 2° be determined systematically correct to, say, $\pm\ 0''{\cdot}05$. The combination of an east and a west elongation is out of the question, as the interval in time between them is too long. We would have to take one elongation, and make the observations round about the epoch of Jupiter's stationary point, so as to be able to use the same comparison stars for a considerable time. It would be necessary to determine the position of the satellite relatively to faint stars in its neighbourhood, and again the position of these faint stars relatively to bright stars in the same region of the sky, and then to determine the distance of these bright stars from Jupiter, or rather

from the inner satellites. Each of these determinations should have a probable error smaller than $\pm 0''{\cdot}03$. The problem would thus ultimately be again reduced to that of determining a distance of about two degrees systematically correct to this limit, *i.e.* to about 1/200,000 or 1/250,000, with two other problems of equal difficulty added on. I should not like to say that this is beyond the range of possibilities, but it would certainly be extremely difficult. And no account has yet been taken of the uncertainty introduced by the lack of accurate knowledge of the elements of the orbit and of the perturbations. To begin with it is tacitly assumed that the period of revolution is exactly known, or, to be precise, that this period, which is about two years, is known with an accuracy of about 10 or at most 20 seconds. I doubt whether this is the case. Then the influence of the other elements is large. The circumstances are very different from those of the inner satellites, where we can combine opposite elongations and the eccentricity is consequently practically eliminated, and where the perturbations are small and either of short period, and consequently eliminated, or (at least in the case of IV.) accurately known. The eighth (or the ninth) satellite would have to be taken practically at only one point of its orbit, and the perturbations are large. I have not gone into the question whether it would be possible from existing data to determine the elements and the perturbations with the requisite accuracy, as I consider the purely observational difficulties already prohibitive. An international campaign would be required, probably of considerably greater extent than for the solar parallax by observations of Eros, and I doubt whether the mass of Jupiter, which is already better known than any other astronomical constant, with only two exceptions * (leaving mean motions out of consideration), will be thought of sufficient importance to warrant the organisation of such a campaign.

II.

Great interest has been attached from the beginning to the system of the four satellites as a replica on a smaller scale of the planetary system. We can compare the four Galilean satellites either with the system of the four inner planets, or with that of the four great outer planets. The ratios of the periodic times are in the two cases :

Ratio of Periodic Times.

Inner Planets.		Outer Planets.	
I. and Mercury	: 50	I. and Jupiter	: 2450
II. „ Venus	: 63	II. „ Saturn	: 3040
III. „ Earth	: 52	III. „ Uranus	: 4380
IV. „ Mars	: 41	IV. „ Neptune	: 3620
Mean	52	Mean	3500

* The exceptions are the constant of precession and the lunar parallax. The mean radius of the earth, the acceleration of gravity, g, and the velocity of light, which are also known with greater accuracy, are not purely astronomical constants.

As regards the ratio of the masses to that of the primary, the satellites are intermediate between these two cases. The interval of 321 years since the discovery of the satellites thus is, in the number of revolutions, equivalent to nearly 18,000 years of the four inner planets, and to more than 1,100,000 of the outer planets. During all this time the general aspect of the system has not changed, and especially the stability appears to be unimpaired. This enormous magnification of the time scale makes the system of the satellites of special interest for the study of secular and long periodic perturbations. It is to be regretted from this point of view that accurate observations are only available for about 40 years, corresponding, however, already to more than twenty centuries of the system of the inner planets.

Beside this magnification of the time scale the chief feature of the mathematical theory of the satellites is the commensurability of the mean motions of the three inner ones. Not only is the period of II. nearly double that of I. and of III. double that of II., but the differences $n_1 - 2n_2$ and $n_2 - 2n_3$ are equal to each other within the uncertainty of their determination, which is about one two-millionth of its amount. The fourth satellite does not partake in this commensurability, and, so far as periodic perturbations are concerned, its theory does not present any particular difficulties, and is in many respects similar to the lunar theory, only much simpler, since the ratio of the periodic time to that of the primary, which is 1/13 in the case of the moon, is only about 1/260 in the case of the fourth satellite. The obvious course would thus appear to be to have one theory for the three inner satellites, and another one for the fourth, but this is impossible, on account of the secular perturbations which are for the four satellites so intimately interwoven that they cannot be separated, and the theory of the *four* satellites must of necessity be treated as one whole. In the classical theory this is done by Lagrange's method, applied to a uniform circular motion as intermediary orbit. In my new theory the same method is used, but it is applied to an intermediary orbit, which includes already the principal inequalities of the three inner satellites. For the fourth satellite this method has some disadvantages as compared with the classical procedure, but these are more apparent than real.

As a consequence of the commensurability of the mean motions, the mean longitudes of the three inner satellites can be expressed in the form $\lambda_i = (c_i - \kappa)\tau + \lambda_{i0}$, τ being proportional to the time, with $c_1 = 4$, $c_2 = 2$, $c_3 = 1$, whilst the value of κ is about 1/69. It is, of course, possible to express λ_4 in the same way, but c_4 will not be a whole number. The three mean longitudes λ_1, λ_2, λ_3 are thus expressed as whole number combinations of two angles τ and $\upsilon = \kappa\tau$, the periods of which are 7·051 and 486·8 days respectively. The two arbitrary constants, which have thus disappeared, reappear in the "libration," by which the angle $\vartheta = \lambda_1 - 3\lambda_2 + 2\lambda_3$ oscillates on both sides of the value 180°, thus $\vartheta + 180° = a \sin \psi$. The period of ψ is about six years. The periods of the inequalities in the motions of the satellites, *i.e.* the deviations of the actual motions from uniform motion in a

circle, are, as a consequence of these relations, divided in four sharply separated groups, the first group having periods of the order of that of τ, *i.e.* shorter than a week for the three inner satellites, and 17 days for the fourth; those of the second have periods of the order of that of v, viz. between 405 and 487 days; the third group is the libration, with a period of six years; the fourth group finally contains the inequalities of very long period, equal to or exceeding the period of revolution of Jupiter round the sun, or 12 years. The inequalities of the first group are of three kinds: (*a*) the equations of the centre, (*b*) the "great inequalities," which have the argument $c_i\tau$, and (*c*) periodic perturbations. Those mentioned under (*b*) are the largest. The arguments, periods, and semi-amplitudes of the inequalities I.*a*, I.*b*, and II. are, for the three inner satellites:

Group.	Argument.	Periods.			Maximum Semi-amplitudes.		
		I.	II.	III.	I.	II.	III.
I.*a*	$c_i\tau - v - \varpi_j = \lambda_i - \varpi_j$	$1^d{\cdot}77$	$3^d{\cdot}55$	$7^d{\cdot}16$	$0°{\cdot}001$	$0°{\cdot}015$	$0°{\cdot}236$
I.*b*	$c_i\tau = \lambda_i + v$	$1^d{\cdot}76$	$3^d{\cdot}52$	$7^d{\cdot}05$	·466	$1°{\cdot}070$	·063
II.	$v + \varpi_j$	405^d to 486^d			·017	·073	·018

In the problem of a central body attended by three planets or satellites of infinitesimal mass, there exist periodic solutions analogous to those investigated and classified by Poincaré and Darwin in the problem of three bodies. The solutions of the first kind, in which the orbits of the three satellites are circles, exist if a linear relation with whole numbers as coefficients exists between the mean motions, *i.e.* if the differences of the mean motions are mutually commensurable. For the existence of those of the second kind, in which the orbits are Keplerian ellipses, it is necessary that the mean motions themselves are commensurable. In both cases certain conditions of symmetry must be satisfied. After a complete period the relative positions of the four bodies return to their initial values, the whole system being rotated through a certain angle ($-2\pi\kappa$ in the present case). If the masses are not zero, both kinds of solutions may continue to exist, if certain conditions between the elements are satisfied. In the solutions of the second kind the eccentricities are then determined in terms of the masses by these conditions. In the case of Jupiter and its three inner satellites the differences of the mean motions are exactly, and the mean motions themselves approximately, commensurable. Consequently either the solution of the first or that of the second kind can be taken as an approximation to the true motion.

The conditions of symmetry mentioned above can be satisfied by sixteen different combinations of the initial positions of the satellites and the perijoves. Some of these, however, would lead to contradictory results, and there are only four different families of orbits. In two of these four κ is positive, and in two negative, in two of them ϑ is zero, and in two it is 180°. It has been proved * that the configuration with κ positive and $\vartheta = 180°$ is the only one giving rise to stable orbits.

* *Proceedings Roy. Acad. Sci. Amsterdam*, 1909 February.

In Laplace's theory the periodic orbit of the first kind is taken as "intermediary orbit," in the new theory that of the second kind. The equations of the centre I.*a* appear as "variations" in both cases. The inequalities I.*b* appear as "perturbations" in Laplace's theory, giving rise to small denominators. In the new theory these inequalities are included in the intermediary orbits, which now consist of Keplerian ellipses having the common retrograde motion $-\kappa\tau$ of the apses. They are the eccentricities of these intermediary ellipses, and are determined from the equations expressing the conditions of periodicity. Once these equations have been solved, all small divisors are done with, and do not reappear in the further developments. Moreover, the "variations" in this case include not only the equations of the centre I.*a*, but also the inequalities of group II. and the libration III. All these are now expressed in the form $\epsilon_q \left\{ \begin{matrix} \sin \\ \cos \end{matrix} \right\} \psi_q$ with $\psi_q = \beta_q \tau + \varpi_{q0}$, where ϵ_q and ϖ_{q0} are constants of integration, whilst $\beta_q{}^2$ are the roots of the determinantal equation, which is well known from Lagrange's method for the secular perturbations.

The success of a theory depends on how much it includes in its first approximation. If we call "first approximation" the intermediary orbit and the variations, then the new theory includes, as has just now been remarked, the inequalities I.*b*, II. and III., which in the classical theory are treated as perturbations. The solution of the equations of condition for the intermediary orbit is very convergent, and the solution of the Lagrangian determinant for the variations is also not more, and probably less, laborious in the new method than in the old, although it has now sixteen (very easily reduced to eight, however) columns and rows instead of four. That we have eight columns and rows, instead of four, is due to the fact that the non-periodic part of the perturbative function includes not only the secular, but also the critical terms, some of which are of the first degree in the eccentricities. That we get an equation in β^2 instead of β, is due to the higher orders of the eccentricities, which are usually neglected.

In comparing with Laplace's theory it should be kept in mind that, although Laplace's determinant has only four columns and rows, his equation is actually of a very high degree, since the unknowns appear not only in the diagonal terms, but also in numerators in all terms.

The remaining perturbations of short period (I.*c*), and the solar and other long-periodic perturbations (IV.) might be treated in the same way as in the ordinary theory, and would then give no additional difficulty. There is, however, a certain charm in the idea of continuing to use the same method throughout, and so I have been tempted to try to treat the perturbations also by the Lagrangian method of variations. This, if it succeeded, would have the advantage of giving at once the corrections to the motions of the perijoves and the argument of the libration due to the perturbations of the second order. The application of the method of variations to the perturbations leads to

an infinite determinant. This can, however, be reduced to one of eight columns and rows by simple operations, though the amount of calculation required is very large. The determinant used for the variations, which is derived from the secular and critical part of the perturbative function only, also had eight columns and rows, but it could be proved that three of its eight roots are necessarily zero. This proof is based on the conditions for the intermediary orbit, viz. the conditions of symmetry and those resulting from the commensurability of the mean motions. The new determinant may have eight roots different from zero. All that can be proved is that three of these are of the second order in the masses. This was realised beforehand, but it was hoped that these three would come out so small as to be practically zero. This hope, however, was disappointed, owing to large numerical factors occurring in the development, and the failure of positive and negative terms to cancel each other. This was the more disconcerting, as one of the roots β^2 came out negative, thus giving rise to an exponential term, $\epsilon_1 . e^{\beta\tau} + \epsilon_2 . e^{-\beta\tau}$, instead of a trigonometrical one, $\epsilon_1 \cos \beta\tau + \epsilon_2 \sin \beta\tau$.

It was thought for a while that this might be due to a want of convergence of the infinite determinant, but some additional computations have led me to believe that the convergence, though slow, is sufficient, and in any case there is not the slightest hope that the particular terms, to which the negative root is chiefly due, will be cancelled out by terms of higher order now neglected. The true explanation must be sought in the mathematics of the method. Mathematics, like justice, is blind, and does not know what it does. Or, rather, it does the rigorously correct thing, even if asked to have consideration to circumstances. Mathematics can have no considerations. Also, of course, it can make no mistakes. The problem (neglecting the inclinations, as we do here) contains sixteen constants of integration. Eight of these refer to the equations of the centre, the other eight to the mean longitudes. The two belonging to the longitude of Satellite IV. need not be further considered ; they give no difficulty. Of the six others, four are in the first approximation eliminated from the problem by choosing the intermediary orbit so that the conditions of periodicity are satisfied, and restricting the perturbative function to its non-periodic part. These are the two constants (the mean motions of τ and of v) involved in the mutual commensurability, and two others implied in the conditions of symmetry. The remaining two appear as the amplitude and phase of the libration. If the complete perturbative function is used, the restriction is removed, and the four extra constants of integration are bound to reappear. In which form they will present themselves depends on the mathematical method chosen, and by our method they are forced to appear as amplitudes and phases of periodic terms, or as coefficients of exponentials. It is to be noted that, if any ordinary planetary theory were treated by the same method, instead of keeping the mean motions and longitudes at the epoch indeterminate as in the usual theory, the same thing would happen. The constants of integration taken out by fixing the mean motions at determined numerical

values (whether mutually commensurable or not) and taking fixed values for the initial longitudes (whether symmetrical or not), would crop up again as arbitrary coefficients and phase angles.

There are several ways in which this difficulty might be avoided. We might, by successive Delaunay transformations, or similar operations, as used by von Zeipel in his theory of the minor planets, eliminate the various terms of the perturbative function *before* the formation of the determinant. There would then be no infinite determinant at all. Or we might, by means of the integrals of energy and of areas, reduce the number of degrees of freedom by two. We would then get an infinite determinant having twelve, instead of sixteen, columns and rows for each periodic argument of the perturbative function, which could be reduced to a central determinant having six, instead of eight, columns and rows. The sixth root, corresponding to the constants of integration removed by fixing the mean motion and longitude at epoch of the fourth satellite, would give no trouble. And there are still other possible methods.

It should be pointed out that all these troubles and difficulties are occasioned not by any complication connected with the special nature of the problem of the satellites, but solely by the high degree of approximation aimed at. They arise from the attempt to determine the effect on the motions of the perijoves and the argument of the libration, of terms of higher order. In the older theories this effect is simply ignored. For the perijoves it is almost certainly entirely negligible. For the period of the libration it may be appreciable, but as the coefficient of the libration is extremely small, it does not matter. Thus, from the practical point of view, the mathematical puzzle might just as well be left unsolved. It has, however, a certain interest as a puzzle, and that is the reason why I have mentioned it.

All different methods must, of course, ultimately give the same values for the *co-ordinates*. The verification of this equivalence of very differently expressed results has some resemblance to the problems with which we used to amuse ourselves in the first days of relativity, verifying the identity of phenomena expressed in different systems of space and time co-ordinates. Only the present puzzle is far more intricate than most of the relativity problems.

The theory of the latitudes has been developed by Dr. Leckie. His treatment, though very exhaustive and thorough, presents no fundamentally novel character. It must, however, still be subjected to some slight numerical revision.

The new theory differs from most others, whether of the satellites or of planets, by its completeness. It is entirely numerical. Numerical values of the mean motions, *i.e.* of c_i and κ, are introduced from the beginning, and these are supposed not to be liable to correction. Also for the masses numerical values are introduced, but indeterminate factors $(1 + \lambda_i)$ are retained, and developments are made in powers of the corrections λ_i. This method makes it feasible to compute every coefficient needed in the theory completely, *i.e.* including the effect of

all terms of the perturbative function, and thus to assure the accuracy of the result to a certain predetermined limit. The intermediary orbit is computed to eight decimal places, and the development in powers of λ_i has been carried so far that, for any corrections to the masses within the limits of possibility, this accuracy is retained. The variations have been computed with a similar accuracy, having regard to the values of the arbitrary coefficients ϵ_q (the " proper " eccentricities). The accuracy of the computed co-ordinates, of course, diminishes with increasing time, owing to the remaining uncertainty of the motions of the variational arguments, but it remains considerably larger than that of the best modern observations for at least two centuries on both sides of the central epoch. Owing to the enormous magnification of the time scale already mentioned this corresponds to an interval of more than a hundred centuries in the theory of the four inner planets.*

III.

As in the mathematical theory, so also in the derivation of the elements from observations, the chief difficulties are introduced by the mutual commensurabilities.

Consider first a series of observations, heliometric, micrometric, or photographic, covering a limited interval of time, say one or two months. From the near equality of the periods of the inequalities (I.*a*) and (I.*b*) it is evident that these are very badly separated, and practically all we can derive from such a series is a linear combination f of the two corrections δh and δk to the equation of the centre (I.*a*) and the correction δx to the coefficient of the great inequality (I.*b*). Moreover, since the satellites are not measured relatively to the planet, but only differences of their co-ordinates are determined, we can only get the differences $f_i - f_j$ of these linear functions. In the discussion of the three heliometric series of 1891, 1901, and 1902 the three unknowns δh, δk, and δx were determined separately, but the weights are small owing to this reason. In order to separate the three unknowns we must combine three widely different epochs. The distance in time between the two epochs 1901 and 1902 is not sufficient to effect an entirely satisfactory separation. In the reduction of the micrometric series of observations taken at Washington and the various photographic series, constant terms were introduced as unknowns corresponding to the linear combinations f_i, or rather their differences. These constants may, besides the unknowns δh, δk, δx, also contain possible systematic errors in the measures or the photographs (guiding errors, etc.).

Next take the combination of the mean results from different series like those discussed above. The correction to the mean longitudes derived from each epoch will contain the corrections to the inequalities

* A general outline of the principles of the theory has been published in *Leiden Annals*, vol. xii, part 1, 1918. Part 2 (1919) contains Dr. Leckie's theory of the secular part of the latitudes, and part 3 (1925) gives the intermediate orbit and the variations, the latter including the terms of the second and third degree. Part 4 will contain the " perturbations."

of long periods, *i.e.* those of the groups II., III., and IV. Now naturally such series will be taken near the dates of opposition of Jupiter, *i.e.* on the average about 400 days apart. The periods of the inequalities of group II. are in the neighbourhood of 480 days, and these inequalities will consequently in a series of such mean results have an apparent period of about 2400 days ($1/400 - 1/480 = 1/2400$) and will thus not be separated from the libration, of which the period is of the same order. As contrasted with the former, this difficulty is not inherent in the system of Jupiter, but is due to our position on the earth. It would not exist, *e.g.*, for an observer on Venus. We can avoid it by making our observations not at the times of opposition, but at shorter intervals. For this reason the series of observations made at the Cape, Greenwich, and Leiden was planned to be taken at epochs from 50 to 100 days before and after each opposition, the interval giving a difference of phase of about 90° in the inequalities of the group II. The original intention was to extend these observations over eight years, two series being completed each year, except in the year 1918·19, when Jupiter's equator was parallel to the earth's, in which year the two series were to be combined to form one strong series for the purpose of determining the positions of the orbital planes. The observations were begun at the Cape in 1913 and were continued at Greenwich after 1916, when Jupiter's declination had become northerly. Owing to various circumstances, chiefly brought about by the War, the observations at Greenwich were interrupted in 1917, and discontinued after 1919. The series was then completed by observations at Leiden in 1922 and at the Cape in 1924.

In eclipses the argument of the inequalities I.*b*, which is $\lambda_i + v$, becomes $L + v$, L being Jupiter's heliocentric longitude. The period consequently becomes 437·6 days and the determination of the coefficient from observations is mixed up with corrections to the inequalities of group II. This again is a complication that is inherent in the system of Jupiter, and cannot be avoided.

I will now communicate the principal results derived from the discussion of the observations. Although these have all been published before, I am very glad to have the occasion to give a general summary of them, considering them in connection with each other and with the theory.

The observations that have been used are the following:—

1. The heliometer observations at the Cape of 1891 (Gill and Finlay).
2. Those of 1901 and 1902 (Cookson).
3. Photographic plates taken at the Cape with the Carte du Ciel telescope in 1891.
4. Photographic plates taken at Helsingfors and Pulkowa from 1891 to 1898 by Donner and Kostinsky with Carte du Ciel instruments, measured by Renz.
5. Photographic plates taken with the Cape Astrographic telescope in 1902, measured by Cookson.

6. Photographic plates taken at the Cape with the same instrument in 1903.
7. Photographic plates taken at the Cape, again with the same instrument, in 1904.
8. Micrometer observations made by various observers at Washington in 1903 to 1908, discussed by Dr. Kremer.
9. Photographic plates taken at Pulkowa with the Carte du Ciel instrument from 1904 to 1910 by Kostinsky, measured by Renz.
10. Micrometer measures made at Berlin from 1907 to 1909, discussed by Dr. Guthnick.
11. The series of plates taken at the Cape with the Victoria telescope in 1913, 1914, 1915, at Greenwich with the Thompson equatorial in 1916 and 1918–19, at Leiden with the astrographic telescope of 516 cm. focal length in 1922 and at the Cape in 1924, all measured and discussed at Leiden.
12. Of this series the plates taken at Greenwich in 1918–19 and at the Cape in 1924 must be separately mentioned, as these were provided with special means for the determination of the orientation by trails with a view to the derivation of the latitudes.
13. Photographic plates taken at the Yale Southern Station at Johannesburg in 1927 and 1928 by Dr. Alden, and measured by him.

The following eclipse observations have been used more or less :—

14. The series of observations by Dr. Innes, Mr. Wood, and other observers at the Union Observatory at Johannesburg from 1908 to 1926. These have been discussed by Dr. Brouwer.
15. The series of photometric observations made at Harvard College Observatory from 1878 to 1903, discussed by Professor Sampson.
16. The discussion of the same observations of Satellite IV. by myself.
17. A series of 83 observations of complete eclipses of Satellite III. by different observers from 1773 to 1799, discussed by me.
18. A series of eclipses of Satellite I. from 1668 to 1745 compared by Wargentin with his tables.
19. A similar series of Satellite II. from 1668 to 1748 compared by Wargentin with his tables.
20. A series of eclipses of Satellite II. from 1668 to 1808 compared by Delambre with his tables.
21. A series of eclipses of Satellite III. from 1668 to 1782 compared by Wargentin with his latest tables.
22. Eclipses of Satellite III. from 1837 to 1898 compared with Damoiseau's tables, collected by Professor Sampson.
23. Part of a collection of eclipses of Satellite IV. from 1671 to 1808 compared by Delambre with his tables. Only the observations from 1777 to 1802, which were discussed by Professor Sampson, have been used.

24. The equation of the centre of Satellite IV. derived from Damoiseau's tables.
25. Finally, the heliometer observations made by Bessel from 1832 to 1839 and by Schur in 1874 and 1880 have been compared with the theory, but the results have not been used, as it was not possible to eliminate the systematic errors introduced by the pointing on the planet.

Of the latitudes two discussions were made, one in 1907 * based on the series mentioned under 1, 2, 3, 5, 6 and 7, and one in 1926 † based on these same, supplemented by 10 and 12, and (in 1929) for the node of III. by 15, 17, 21 and 22.

The inclinations and nodes of the planes of the orbits referred to Jupiter's orbit are given by the formulae

$$I_i \begin{Bmatrix} \sin \\ \cos \end{Bmatrix} N_i = \sum_j \sigma_{ij}\gamma_j \begin{Bmatrix} \sin \\ \cos \end{Bmatrix} \theta_j + \mu_i \omega \begin{Bmatrix} \sin \\ \cos \end{Bmatrix} \theta,$$

where ω and θ are the inclination and node of Jupiter's mean equator, and the factors σ_{ij} and μ_i are derived from the theory. The following values of the unknowns were found, those of Souillart's theory, which depend on Damoiseau's tables, being added for comparison :—

	Souillart.	1907.	1926.
ω	3°·0680	3°·1153 ± °·0014	3°·1199 ± °·0010
θ	315·410	315·800 ± ·025	315·859 ± ·018
γ_1	0°·0013	0°·0272 ± °·0028	0°·0317 ± °·0014
γ_2	0·4694	0·4683 ± ·0016	0·4668 ± ·0009
γ_3	0·1789	0·1839 ± ·0026	0·1788 ± ·0010
γ_4	0·2254	0·2536 ± ·0023	0·2452 ± ·0009
θ_1	99°·8	60°·2 ± 7°·0	64°·7 ± 2°·6
θ_2	273·32	293·16 ± ·19	292·81 ± ·11
θ_3	330·59	319·71 ± ·52	319·83 ± ·31
θ_4	5·79	11·96 ± ·67	12·79 ± ·21
$d\theta_1/dt$	−0°·14105	−0°·12613 ± °·00291	−0°·13214 ± °·00062
$d\theta_2/dt$	−0·032974	−0·032262 ± ·000088	−0·032599 ± ·000027
$d\theta_3/dt$	−0·006983	−0·007310 ± ·000232	−0·007216 ± ·000079
$d\theta_4/dt$	−0·001863	−0·001909 ± ·000111	−0·001753 ± ·000049

The average residuals are for the four satellites :

	I.	II.	III.	IV.
Souillart	±0°·0534	±0°·1706	±0°·0430	±0°·0578
1908	± ·0126	± ·0103	± ·0056	± ·0046
1926	± ·0069	± ·0045	± ·0049	± ·0044

* *Cape Annals*, **12**, 5, 1914. † *B.A.N.*, 102, 1926.

By the discussion of 1929 the motion of the node of III. was altered to

$$\frac{d\theta_3}{dt} = -\,0^\circ{\cdot}007084 \pm {}^\circ{\cdot}000027.$$

In this discussion also an independent determination was made of the term $\sigma_{34}\gamma_4$ in the inclination of III. due to the attraction of IV. The correction to the assumed (theoretical) value was found to be insensible. The probable error as derived from the observations is 1/40 of the value of the coefficient.

Weights were assigned in this solution corresponding to a p.e. of unit weight of $\pm\,0^\circ{\cdot}0100$. The p.e. of unit weight derived from the residuals was $\pm\,0^\circ{\cdot}0155$ for the eclipses and $\pm\,0^\circ{\cdot}0106$ for the modern observations. The p.e. given for $d\theta_3/dt$ and σ_{34} take account of this. Still they may not be a true measure of the real accuracy owing to the possibility of systematic errors of the eclipses, which may have appreciably affected the result without increasing the residuals.

It is, in fact, certain that the eclipse observations are affected by very large systematic errors. Nevertheless the value of the node of III. derived from the ancient eclipses has been accepted. Only complete eclipses, of which the disappearance and reappearance are observed by the same observer, have been used. A systematic error different for *in* and *ex* will, if it is *constant*, have no influence on the determination of the node. It will also have no influence if it varies in some regular way with the distance of the satellite from the node, provided the law of variation is the same for *in* and *ex*, *e.g.* proportional to the duration of the change of light from full to invisible. A systematic error, different for *in* and *ex*, depending on the altitude of Jupiter above the horizon will, since all observations were made at nearly the same northern latitude, on the average have the same period as the longitude of Jupiter, and the same is true of an error depending on the season of the year in which the observations are made. Such errors will, in the observed duration of the eclipse, produce a term with a period of about twelve years, whilst the determination of the latitudes depends on terms of half this period. The influence of such errors will thus be very much smaller in the case of the latitudes than for the equations of the centre, which depend on a period of twelve years. On the whole it seems probable that the determination of $d\theta_3/dt$ from the old eclipses, compared with the modern observations, is entitled to a much larger weight than that from the modern observations alone, though it is probable that the p.e., $\pm\,{\cdot}000027$, given above should be somewhat increased to take account of the possible influence of systematic errors which do not affect the residuals, as has been remarked above. In fact I feel inclined almost to double it.

The same cannot be said for Satellite II. for the following two reasons. First, in this case the modern observations embrace almost two complete revolutions of the node, as against about 80° in the case of θ_3, and, secondly, there are practically no complete eclipses of II., so that to derive the semiduration, immersions observed before opposition must be combined with emersions observed after opposition, and

the influence both of systematic errors and of the uncertainty in the other elements is enormously greater than for Satellite III.

The inclinations and nodes, which used to be considered as the most difficult elements to determine, are now the best determined of all the elements. The determination of the equations of the centre and of the great inequalities is much less precise, owing to the complications introduced by the commensurability, as explained above.

In 1908 Dr. Innes started a series of observations (No. 14, p. 719) at the Union Observatory, which is still being continued. The original intention was to observe not only the eclipses, but also occultations, transits, and shadow transits. Thus the satellites would be observed not only in the points of their orbits in opposition to the sun or the earth, but also in conjunction, by which the conditions for the determination and the separation of the different unknowns would be greatly improved. Further, a strong magnification was to be used, so that the discs would be clearly seen and the progress of the phenomenon could be followed in detail. It was hoped that the systematic errors of ordinary eclipse observations would thus be avoided. Dr. Innes seems, however, very soon to have satisfied himself that also the phase observations are not free from systematic errors, due to irradiation and perhaps other disturbing causes. At any rate he has, with the exception of the first two years, concentrated his efforts on the observation of the last and first specks of light in eclipses. The observations from 1908–1926 were reduced and discussed by Dr. Brouwer.*

The determination of the equations of the centre from extra-eclipse observations was based on the series 1, 2, 4, 7, 8, 9, 11. To these were added 16, 17, 24. The results of this discussion were published in 1928.† Dr. Brouwer and myself were working at these two investigations entirely independently of each other, and without knowing much of each other's progress. We happened to reach results at about the same time, and when we came to compare them we were both struck by their almost absolute agreement, which naturally increased the confidence of each of us in his own results. A further investigation was made in 1929, based on the same data, replacing, however, 24 by 23, and adding 21 and 22, as will be explained in a moment.

As for the inclinations, I give again the comparison of the different solutions with the figures derived from Damoiseau's tables. The results of 1908 depend on the Cape heliometer observations (series 1 and 2) only.

	Damoiseau.	1908.		1928. Extra-eclipse Observations.		1928. Johannesburg Eclipses (Brouwer).	
	°	°	°	°	°	°	°
$2\varepsilon_1$	..	0·0062±	·0160	0·0013±	·0007	0·0016±	·0012
$2\varepsilon_2$	..	·0344±	80	·0150±	15	·0152±	20
$2\varepsilon_3$	0·1509	·1736±	130	·1593±	26	·1596±	40
$2\varepsilon_4$	0·8362	·8528±	40	·8436±	10	..	

* *Leiden Annals*, **16**, 1, 1928. † *Ibid.*, **16**, 2, 1928.

	Damoiseau.	1908.		1928. Extra-eclipse Observations.		1928. Johannesburg Eclipses (Brouwer).	
	°	°	°	°	°	°	°
$\tilde{\omega}_1$	..	156	±360	75	±22	22	±40
$\tilde{\omega}_2$	..	63	± 10	149·5	± 4·5	162·5	± 6·0
$\tilde{\omega}_3$	347·8	338·3	± 3·0	345·6	± 0·8	348·7	± 1·4
$\tilde{\omega}_4$	287·15	283·15	± ·30	282·79	± 0·07	..	

In comparing the probable errors of the final values (1928, extra-eclipse observations) with those of the final inclinations and nodes, it will be seen that the accuracy of the latter is considerably greater. This is entirely due to the influence of the commensurability on the separation of the unknowns. It should be noted that the determination of ϵ_1, ϵ_2, ϖ_1, ϖ_2 in both discussions of 1928 depends more on the inequalities of group II. than on the equations of the centre (I.*a*) themselves.

The motion of the proper perijove ϖ_3 of Satellite III., and the equation of the centre $\tau_{34}\epsilon_4$ of the same satellite produced by the action of IV., are amongst the data from which the mass of IV. must be derived. It was with a view to the determination of these quantities, especially of τ_{34}, that the series of complete eclipses mentioned under 17 above was originally collected by me from the literature of the time, and compared with my theory of 1908. Afterwards also the series 15, 21, and 22 were included in the discussion. During the eighteenth century the equation of the centre of Satellite III. had been a great stumbling-block to the astronomers who tried to represent the motions of the satellites in tables. At the end of the seventeenth and the beginning of the eighteenth century the eccentricity was large, the coefficient in the longitude being of the order of a quarter of a degree; towards the end of the eighteenth century it had decreased to about a tenth of a degree, but appeared to be increasing again. Wargentin in his tables of 1771 introduced two inequalities with different amplitudes and different arguments, but in his new tables * he replaced these by a single inequality with varying amplitude. In the meantime the problem had been solved analytically by Lagrange, who showed that in consequence of the mutual attractions each satellite partakes of the equation of the centre of all others, so that there are actually four equations of the centre to each satellite. Those due to the action of I. and II. on III. are, however, negligible, so that the representation by two sine terms, as in Wargentin's tables of 1771, is indeed theoretically correct. The coefficients of Wargentin's two inequalities also are approximately correct.

* The date of these tables is not certain. They were not published, but they were found amongst Wargentin's manuscripts kept in the library of the Academy of Sciences of Stockholm. Professor Lindblad kindly sent me a copy of them. The MS. probably was written about 1780. The improved tables of III., mentioned by Houzeau as being published in London in 1779, do not exist. Those contained in the collection of astronomical tables published by the Berlin Academy in 1776, which are referred to by Wargentin as "Editio Berolinensis," are a reprint of the tables of 1771, with some additions by Bode.

He had, however, derived the motions of the arguments of these two inequalities empirically, and the motion of the smaller of the two, which should actually be that of the proper perijove of IV., was taken between five and six times too large, that of the first argument being much too small. This had the consequence of making the perijove retrograde after about 1770, and this probably was the reason why he rejected this representation and replaced it by a single equation of the centre with a variable amplitude. This, in fact, represents the observations much better than his first tables, and also better than the tables of either Delambre or Damoiseau, which were based on the theory of Lagrange and used approximately correct motions of the arguments.

The expression of the total inequality in longitude is

$$2e_3 \sin (\lambda_3 - \pi_3),$$

and we have

$$2e_3 \begin{Bmatrix} \sin \\ \cos \end{Bmatrix} \pi_3 = a_3 \begin{Bmatrix} \sin \\ \cos \end{Bmatrix} \varpi_3 + a_4 \begin{Bmatrix} \sin \\ \cos \end{Bmatrix} \varpi_4.$$

In my discussion I introduced as unknown the corrections

$$\begin{aligned} \alpha &= \Delta a_3, \\ \beta &= a_3 \Delta \varpi_3 (1900), \\ \gamma &= a_3 \Delta (d\varpi_3/dt), \\ \delta &= \Delta a_4, \end{aligned}$$

the argument ϖ_4 being taken from the discussion of the observations of the fourth satellite. I found it impossible to represent the ancient observations (series 17, 21 and 22) and the modern ones by one and the same formula. The two sets of values found for the unknowns were : *

From Modern Observations alone.	From Modern Observations and old Eclipses.
$\alpha = 0 \quad \pm \cdot 003$	$\alpha = -\cdot 005 \pm \cdot 002$
$\beta = 0 \quad \pm \cdot 004$	$\beta = +\cdot 010 \pm \cdot 002$
$\gamma = -\cdot 031 \pm \cdot 015$	$\gamma = +\cdot 021 \pm \cdot 002^5$
$\delta = 0 \quad \pm \cdot 004$	$\delta = +\cdot 009 \pm \cdot 002$

The two sets are irreconcilable ; we must choose either the one or the other. I have finally decided to adopt the first set. The second would leave large residuals in the representation of the modern observations, for which no explanation can be found. The first solution leaves large residuals in the old eclipses, but these are of the nature of systematic errors depending on the zenith distance of Jupiter, or on the season of the year, having roughly the period of Jupiter's time of revolution. In fact it was found possible to represent the old observations very satisfactorily by assuming a systematic error of this kind. The confidence in the reliability of the old observations is not increased if we consider the widely different representations derived from the same observations by different investigators. The values of the coefficients a_3, a_4 and the arguments ϖ_3, ϖ_4 used in different theories are given

* *Leiden Annals*, **16**, 4, p. 43, 1929.

below. Those given under the heading "Warg. + mod." result from a special investigation based on the same modern data as the discussion of 1928 (extra-eclipse observations) related above and Wargentin's eclipses (21). The time is counted in years from 1750·0.

	a_3.	a_4.	ϖ_3.	ϖ_4.
Wargentin, 1771	0°·157	0°·087	245°·3+1°·582T	350°·1+4°·030T
Wargentin, 1780	*variable*	..	287·5+2·493	..
Delambre	0·154	0·068	309·4+2·625	(180·3+0·730)
Damoiseau	0·151	0·038	315·0+2·673	(180·4+0·708)
Sampson	0·174	0·074	318·5+2·548	(179·3+0·693)
Modern	0·159	0·077	319·2+2·572	(179·7+0·688)
Warg.+Mod.	0·160	0·102	285·8+2·665	(178·8+0·693)

In all of these, except the first, the value of ϖ_4 is, of course, taken from the theory of the fourth satellite. The variable value of a_3 of Wargentin's manuscript tables of 1780 is

before 1720 . . .	0°·244
1720–1728 . . .	0·209
1728–1734 . . .	0·183
1734–1747 . . .	0·157
1747–1754 . . .	0·122
1754–1780 . . .	0·087,

and he remarks that it appears to be increasing again after 1780. The diagram (fig. 1) shows the representation of the observations in the seventeenth and eighteenth centuries by these formulæ. The semi-major axes of the long ellipses represent twice the probable error as derived from the observations. The last point is that derived from the eclipses from 1777 to 1799 (17), the other nine are Wargentin's eclipses (21). "Sampson", which does not differ much from "Modern", has been omitted from the diagram. For the perijove all representations are about equally good (or bad). The eccentricity is much better represented by either of Wargentin's formulæ than by Delambre or Damoiseau. But it should be remembered that these are observations collected and compared with his tables by Wargentin himself. Delambre and Damoiseau may have used partly different observations, and perhaps somewhat differently reduced. Wargentin mentions expressly that no observation has been excluded from this collection on account of discordance with the tables. It should be noted that in the reduction of the observations of Wargentin's collection the modern values of the inequalities of the groups I.*b* and II. have been used.

I have lingered rather long on this question of the equation of the centre of III., because it is one of the few points on which an entirely satisfactory solution of all difficulties has not been reached. We now come to the consideration of the mean longitudes.

The discussion of the longitudes, of which the results were published in 1928, depended on the data 1, 2, 4, 7, 8, 9, 11, 16, and 17, and the epochs for 1750·0 taken from Damoiseau's tables. It was found that the observed longitudes of the satellites could not possibly be represented by a purely linear formula, without taking into account the

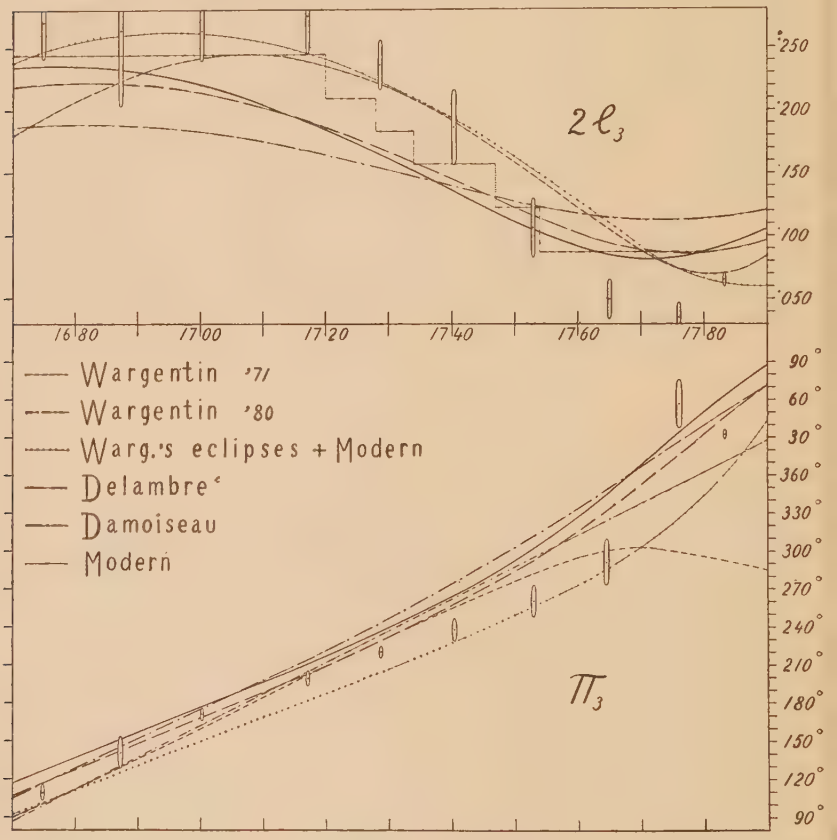

FIG. 1.

correction from astronomical to "Newtonian" or uniform time. For this latter the formulæ derived in *B.A.N.*, 124 and 127 (1927), from a discussion of the longitudes of the moon, the sun, Venus, and Mercury, were used.* Even then large residuals remained. These could be considerably reduced either by a positive correction to the adopted average secular retardation of the rotation of the earth, or by a positive correction to the ratio Q of the fluctuations in the rotation of the earth

* See Appendix to Report of Commission 7 of the I.A.U., *Transactions of the I.A.U.*, 1928, p. 40.

to those in the longitude of the moon, or again by empirical secular terms in the longitudes of the satellites, or, of course, by a combination of these three.

There may, of course, be an effect of tidal friction on Jupiter, like that of the moon on the earth. This, however, would give a *negative* * coefficient of the square of the time in the longitudes of the satellites, and all evidence points to positive secular accelerations (except, perhaps, for the fourth satellite). It is thus evident that no tidal friction acts in the system of Jupiter, or, if it acts, it is extremely small, of an order of magnitude less than 1/10,000 of that acting on the earth.

In 1928 I adopted finally the correction Δt as derived from the moon, sun, and planets, combined with empirical secular accelerations in the mean motions of the satellites. It was chiefly to test the reality of these empirical terms that the discussion of the old eclipses (data 18 to 23) was undertaken. In the meantime also the observations at the Yale Southern Station had become available, and these did not confirm the empirical terms derived in 1928. The labour of collecting old observations from the literature of the time, discussing them, and comparing them with the modern theory was not to be thought of. I thus confined myself to such series of observations which had already been compared with existing tables. The difference of the mean longitude according to the tables and according to the theory to which corrections were sought had then to be determined, and also systematic corrections to the observations themselves. These latter were determined exclusively from eclipses which had been observed by more than one observer, and are thus entirely independent of the tables with which they have been compared. They are, of course, relative to a "mean observer," and not to some absolute standard. They are sometimes very large, and different for disappearances and reappearances, but there can be no reasonable doubt as to their reality, at least of those that are not too weakly determined. Observations for which no systematic errors could be derived have been rejected. The epochs 1750 from Damoiseau's tables were, of course, no longer used in the new discussion.

It used to be considered that the mean longitudes were the elements of all others for which the observations of eclipses would give specially trustworthy determinations. These large systematic errors prove, however, that no observations of eclipses can determine the time within a limit of uncertainty at least of $\pm$ 10^s, and this limit cannot be brought down by increasing the number of observations. Dr. Brouwer has shown convincingly that also the Johannesburg observations of eclipses are affected by similar systematic errors, the observations by discs in no smaller measure than those of the last and first speck of light, and the same is true of the photometric observations at Harvard Observatory. These errors are of the order of magnitude of about one-tenth of the

* The longitude of the moon has an apparent positive acceleration, but this is only because the rotation of the earth is retarded more than the mean motion of the moon. An observer studying the system earth-moon from outside would notice a secular retardation of the moon's mean longitude.

diameters of the satellites. They are also of the same order of magnitude as the differences between the "Harvard readings" and the "Durham readings," *i.e.* between two independent interpretations of the same observations.

As was to be expected, the representation of the modern observations by the solution based on the extended material, including the old eclipses, is not better than by the solution of 1928. It is even considerably worse, as a consequence of the rejection of the empirical secular terms adopted in 1928. Besides corrections to the longitude at the epoch and the mean motions, corrections to the ratio Q and to the secular retardation of the earth's rotation were introduced. These two unknowns could now be satisfactorily separated, thanks to the added old epochs.

For the secular acceleration of the sun's mean motion we have the following determinations taken from *B.A.N.*, 124 (1927) :—

From the sun	$+ 1''\cdot80 \pm 0''\cdot16$
transits of Mercury	$+ 1''\cdot55 \pm 0''\cdot10$
Adopted	$+ 1''\cdot65 \pm 0''\cdot15$

We now find from the satellites :

By the discussion of 1928	$+ 1\cdot83 \pm 0\cdot19$
,, ,, ,, 1929	$+ 2\cdot16 \pm 0\cdot21$

The value 1·65 has been retained. The determinations from the satellites are in reality more uncertain than is expressed by the probable errors. They depend on the three inner satellites only, the fourth satellite giving a negative secular term. There may be real secular accelerations in the system of Jupiter, not yet elaborated in the theory.

For the ratio Q we have :

From the sun (R.A.), total fluctuation	$1\cdot31 \pm 0\cdot13$
(decl.) * minor fluctuations	$1\cdot32 \pm \cdot20$
(decl.) * total fluctuation	$1\cdot10 \pm \cdot22$
Venus, total fluctuation	$1\cdot26 \pm \cdot06$
Mercury, "great empirical term"	$1\cdot32 \pm \cdot02$
minor fluctuations	$1\cdot34 \pm \cdot02$
total fluctuation	$1\cdot19 \pm \cdot08$
Satellites, 1929, total fluctuation	$1\cdot49 \pm \cdot24$
Adopted mean (*B.A.N.*, 124)	$1\cdot25 \pm \cdot08$

I have retained the value adopted in *B.A.N.*, 124. For each of the satellites a correction to the mean motion and the longitude at epoch was determined, without any further empirical secular terms. The representation of the observations will be seen from fig. 2. The

* The value $1\cdot32 \pm \cdot20$ is given by Dyson and Cullen, *M.N.*, **89**, pp. 256 and 549, 1929. The value $1\cdot10 \pm \cdot22$ was derived by me from the data given by Dyson and Cullen. The other determinations from the sun and planets are from *B.A.N.*, 124, 1927.

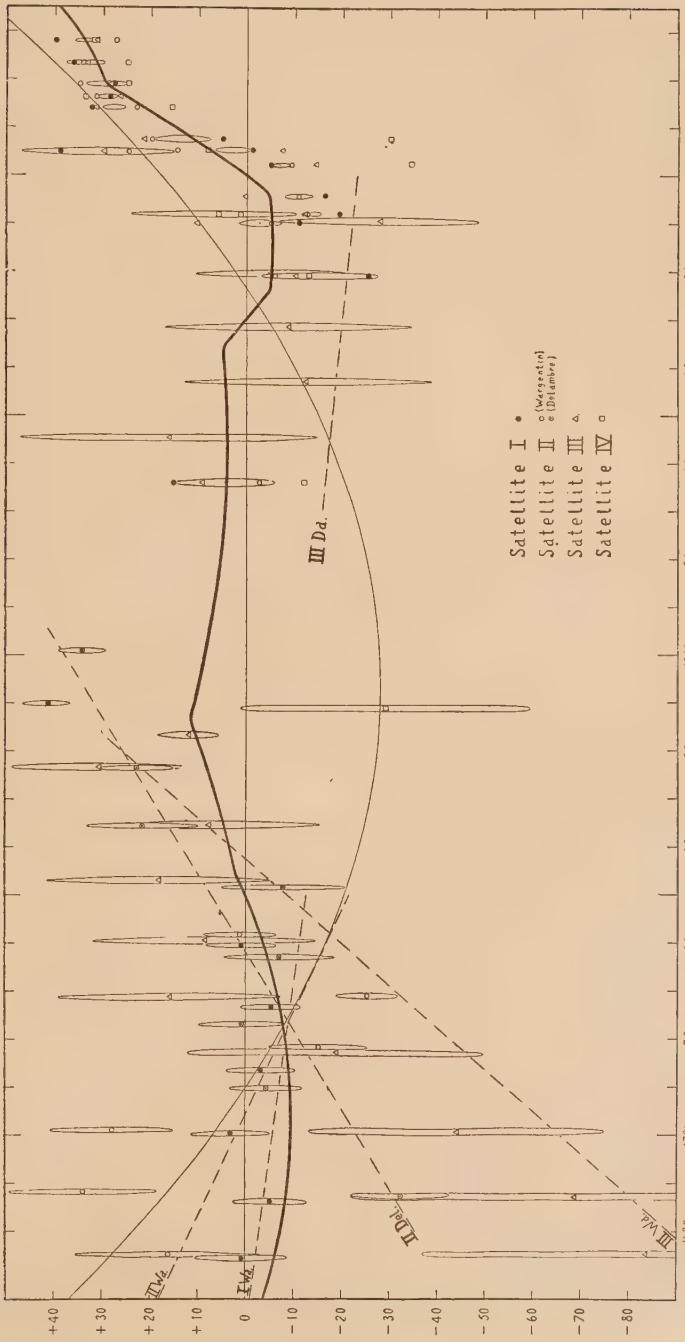

FIG. 2.—Representation of mean longitudes.

full-drawn line is the correction from astronomical to uniform time, with a linear term added, so as to get a more convenient representation on the page, thus $(\Delta t)' = \Delta t + 2^s{\cdot}7 + 42^s{\cdot}5(T + 1)$, T being the time in centuries from 1900. It will be seen that $(\Delta t)'$ never exceeds $\pm 10^s$ previously to about 1906, and is smaller than $\pm 5^s$ from 1820 to 1904. This explains why Dr. Cowell could not possibly have detected the secular acceleration of the sun's mean motion earlier than he did, viz. in 1906.

The major axes of the long ellipses represent 1·5 times the probable error of the mean of the four (or 3, or 1, as the case may be) satellites.

The sudden changes in 1896 and in 1918 or 1919 are strikingly confirmed by the observations of the satellites. The total change between these dates is larger according to the satellites than by the curve, corresponding to the large value of Q.

The broken straight lines drawn in the figure represent the values of $(\Delta t)'$ corresponding to the mean motions of the eclipse tables, compared with the final solution. It will be seen that, especially in the case of Satellite II., Delambre, and III., Wargentin, the observed eclipses conform much more nearly to the tables with which they have been compared than to the curve of $(\Delta t)'$. For Satellite II., where we have partly the same observations compared with two different tables, this is very striking. I can think of no other explanation of this curious fact than, probably unconscious, selection of the observations by the authors of the collections which have been used, though Wargentin states expressly in his paper on the observations of Satellite III. that no observation has been rejected for discordance with the tables alone. It might be thought possible, especially in the case of Satellite III., of which also the equation of the centre from the old eclipses is in strong contradiction with that according to the modern observations, that a real change in the elements of the satellite may have taken place, owing to perturbations by an unknown comet, or some other unexplained cause. This hypothesis, however, becomes very improbable by the fact that the different tables do not only in the old time deviate from the theory as extrapolated from the modern observations, but they disagree quite as much amongst each other.

It will be seen from the figure that there are some very large residuals. If the different equations of condition (of the modern observations, 1891 to 1928) are multiplied by factors so as to reduce them to a nominal probable error of $\pm$ 0·10, then the average residuals left by the different solutions are :

Satellite.	I.	II.	III.	IV.
Marth.	+2·49	±1·24	±·50	—1·61
1908	± ·67	± ·73	±·61	± ·74
1928	± ·19	± ·18	±·15	± ·15
1929	± ·47	± ·25	±·23	± ·18

Where all residuals have the same sign, this sign is given, otherwise the sign $\pm$ is prefixed. The average residuals of the solution of 1929 are much larger than those of the solution of 1928, owing to the rejection

of the empirical secular terms. If the values of the secular acceleration and of Q found from the satellites themselves were used instead of the adopted values from the sun and planets, the residuals for the Satellites I., II. and III. in the solution of 1929 would be reduced to about $\pm$ 0·15.

It is difficult to find a satisfactory explanation of these large residuals. Although they are of an order of magnitude considerably smaller than the systematic errors of the eclipse observations, they are much too large to be ascribed to accidental errors—there are too many of them—or to the probable errors being underestimated, for they occur indiscriminately in the series with small and with large weight, and show no correlation with the accuracy of the observations. As has already been remarked, they may be much diminished by empirical secular (or long-periodic) terms, but it is doubtful whether such terms can be corroborated by the theory. They are most serious in the first satellite, where the existence of appreciable terms of this nature hitherto overlooked in the theory is least probable. A possible explanation, of course, is by variable dark markings on the discs of the satellites, making the centre of light different from the centre of gravity, but this is rather a make-shift explanation, so long as it cannot be verified. A *constant* marking would, of course, produce a spurious inequality of a short period, which would either be absorbed in the equation of the centre or be eliminated in the mean of a series. After all it should not be forgotten that we are dealing with extremely minute quantities, only brought to light by the excellence of the observations and the extreme magnification of the time scale in the system of the satellites as compared with the planetary system. The empirical terms with t^2 adopted in 1928 have not been confirmed by the later investigation, but they may serve as an indication of the order of magnitude of the discrepancies between the observations and the theory. If these empirical accelerations are expressed in the time measured in the periods of revolution of the respective satellites as unit, they are of the order of magnitude of about one hundredth of the secular acceleration of the sun's longitude (due to the retardation of the rotation of the earth) similarly expressed.

The libration is very difficult to determine. The period depends on the adopted values of the masses of I., II. and III., and is very sensitive to a small change in these, especially in m_1, which is the most uncertain of the three. It was hoped at one time that the period could be determined from the observations, but the amplitude is so small that a reliable determination is impossible. In 1907 I found for the coefficient 0°·158 $\pm$ 0°·033, assuming the period. The probable error is greatly underestimated. In 1928 a new discussion was made, in which the amplitude, period, and phase were determined from the observations. The result was

$$(a) \qquad \vartheta = 0°{\cdot}0247 \sin (288° + 0°{\cdot}1658t)$$
$$\pm \quad 75 \quad \pm 15 \pm \quad 32$$

The time is counted in days from the mean date of the series 1913–24 (11). A separate determination from Satellite I. gave nearly the

same phase, but about four times the coefficient, while Satellite II. gave the same coefficient but a difference of 100° in phase. The determination is thus extremely uncertain. The average residuals are only diminished by the solution in the ratio of 4 to 3. This determination was based on the observations 1, 2, 4, 7, 8, 9, 11. The observations (13) at the Yale Southern Station were not then available. It appears that those of 1927 agree exactly with the value (*a*) of ϑ, but those of 1928 differ from it by five times the probable error. The conclusion must thus be that the libration is too small to be as yet detected by the observations.

It is, however, a remarkable fact that Dr. Brouwer, in trying to determine the libration from the Johannesburg eclipse observations found practically the same value, viz. :

$$(b) \qquad \vartheta = 0°{\cdot}0309 \sin (295° + 0°{\cdot}1712t) \pm 58 \quad \pm 11$$

Dr. Brouwer did not try to determine the period independently, which is the explanation why his probable errors of the amplitude and phase are smaller than those of (*a*). He also found that the separate investigation of the Satellites I. and II. gave contradictory results, and he also concluded that the determination only showed the libration to be too small to be determinable from the observations. The agreement of the two absolutely independent determinations (*a*) and (*b*), by different investigators and different methods from different observations, is, however, very remarkable.

I consider it unsafe to draw from this agreement the conclusion that the libration is, after all, not immeasurably small and has actually been determined by Dr. Brouwer and myself, but *if* this conclusion were accepted, we would be forced also to admit the further conclusion that the remaining large residuals in the mean longitudes, which are responsible for the contradiction between the determinations from the Satellites I. and II. separately, are also real and not due to the observations. For, if there are unknown small secular or long-periodic perturbations, they will by the mutual attraction of the satellites be so co-ordinated as to satisfy the same linear equation as the mean longitude, viz. $\delta\lambda_1 - 3\delta\lambda_2 + 2\delta\lambda_3 = 0$, and will consequently disappear from ϑ, although affecting the separate satellites. On the other hand, it is not to be expected that observational errors will obey the same relation.

The following table contains the data derived from the observations, which have served at one time or another for the determination of the masses. Laplace used the data 2, 7, 9, 10, 11. Adams used the same data taken from Damoiseau's tables. I myself have tried to make determinations of the masses at various times, some of which have been published and others not.

Data for Determination of the Masses.

No.	Symbol.	Datum.	Used by.	Observed Value.	Residual.
1	$d\theta_1/dt$	Motion of node of I.	(3)	− °·13213 ± °·00070	+ °·00067
2	$d\theta_2/dt$	,, ,, II.	L, (1), (2), (3)	− ·032599 ± 030	+ 004
3	$d\theta_3/dt$	,, ,, III.	(1), (2), (3)	− ·007084 ± 050	− 111
4	$d\theta_4/dt$	,, ,, IV.	(3)	− ·001753 ± 050	+ 1
5	$d\tilde{\omega}_2/dt$	,, perijove of II.	(3)	+ ·0401 ± ·0015	+ ·0002
6	$d\tilde{\omega}_3/dt$	,, ,, III.	(3)	+ ·00704 ± 20	− 07
7	$d\tilde{\omega}_4/dt$	,, ,, IV.	L, (1), (2), (3)	+ ·001884 ± 020	− 011
8	$\sigma_{34}\gamma_4$	Incl. of III. due to IV.	(3)	0·0295 ±·0015	− ·0021
9	$\tau_{34}\varepsilon_4$	Eccen. of III. due to IV.	L, (3)	0·0771 ± 50	− 52
10	x_1	Great inequality of I.	L, (1), (2), (3)	0·4657 ±·0015	− ·0006
11	x_2	,, ,, of II.	L, (1), (2), (3)	1·0694 ± 20	+ 05
12	x_3	,, ,, of III.	(1), (2)	0·064 ± 5	− 3
13	$d\psi_5/dt$	Period of libration	(1), (2)	0·1658 ±·0100	+ ·0084

In the table the numbers (1), (2), (3) refer to my determinations of 1908,* 1918,† and 1931.‡ The observed values, as derived from the results of my last discussion, are given with their estimated uncertainties. These uncertainties are not meant as probable errors. They are in most cases considerably larger than the probable errors as derived directly from the observations, and they may perhaps in some cases have been overestimated. The residuals given are those remaining after substitution of the masses of 1931 (3). It should be kept in mind that the theoretical expressions are in some cases not quite final, especially so for the latitudes (data 1 to 4 and 8) where the numerical work stands in need of revision. This may be the explanation of the large residual of $d\theta_3/dt$. Also the theoretical expression for the last datum, the period of the libration, is still subject to some uncertainty, but this is smaller than the uncertainty of the observed value.

The following table gives the values of the masses as determined by different investigators at different times, expressed in the mass of Jupiter as a unit. I have added the coefficient Jb^2, expressing the dynamical effect of the compression of the planet. J is a pure number, defined by a certain definite integral, and independent of the units; b, the equatorial semidiameter of the planet, does of course depend on the unit of length; it is here expressed in the unit used in my theory, which is in the neighbourhood of the semi-major axis of the orbit of the third satellite. The observations give only Jb^2 and not J.

The probable errors given with the last determination depend entirely on judgment and are meant as true probable errors according to the definition of this term. Thus, *e.g.*, in the case of m_4 the intention is to convey my conviction that the probability of the true value being between the limits 0·0000469 and 0·0000549 is one half. Probable

* *Proceedings, Amsterdam*, **10**, p. 719, 1908.
† *Annals, Leiden*, **12**, 1, 1918.
‡ Unpublished.

Masses adopted by Various Authorities.

	$10^4 Jb^2$.	$10^4 m_1$.	$10^4 m_2$.	$10^4 m_3$.	$10^4 m_4$.
Laplace	1·056	0·173	0·232	0·885	0·427
Damoiseau	1·057	0·169	0·232	0·885	0·425
Adams	1·044	0·313	0·232	0·812	0·215
Sampson	1·012	0·450	0·254	0·799	0·450
(1) 1908	1·032	0·260	0·231	0·804	(0·425)
(2) 1918	0·993	0·380	0·254	0·820	0·452
(3) 1931	1·002	0·381	0·248	0·817	0·509
	±10	±30	±5	±10	±40

errors derived from the residuals by the rules of least squares would have been much smaller. The masses are of the order of magnitude of that of our moon, and so are the diameters. The determinations of the latter are, however, affected with considerably greater uncertainty than those of the masses. Still it appears probable that the satellites are bodies of a similar constitution to our moon.

It is interesting to state the uncertainty in the computed co-ordinates corresponding to the probable errors of the elements. These are given below for the two co-ordinates x and y, respectively perpendicular and parallel to the projection on the celestial sphere of a line perpendicular to the adopted fundamental plane, or mean equator of Jupiter. For the x co-ordinates the uncertainty due to scale, *i.e.* to the mass of the system, is given separately. It is much smaller than that due to the uncertainty of the elements, which in this case are the mean longitude and the inequalities I.*a*, I.*b*, and II.

Uncertainty of Co-ordinates by Elements 1929.

Satellite.	Latitudes.	Distances	
		by Scale (M).	by Elements.
I.	$\delta y = \pm 0''{\cdot}003$	$\delta x = \pm 0''{\cdot}001$	$\pm 0''{\cdot}010$
II.	± ·003	± ·002	± ·012
III.	± ·005	± ·003	± ·020
IV.	± ·009	± ·005	± ·020

The accuracy with which the position of a satellite can be measured on a plate relatively to the stars is very much larger than that attainable in the measures of the planet. The relative position of the satellite and the planet being capable of practically rigorous determination, as is apparent from the above figures, it follows that the position of Jupiter relative to the stars can be determined with great precision through measures of the satellites. Measures of the satellites relative to stars can thus be used either to determine the place of Jupiter with a view to the improvement of its orbit, or to determine the periodic errors ($\Delta\alpha_3$) in the positions of the stars by comparison with Jupiter. This

idea was suggested, and is being carried out, by Professor Schlesinger, who, I think, will permit me to mention it here.

It is interesting to compare the accuracy of the different kinds of observations. We have discussed photographic plates taken with four different focal lengths. For the Carte du Ciel type of telescope I have also used the data derived from the many plates (all taken at Helsingfors) for parallax and proper motion discussed by me at the Groningen Laboratory. In the following table are given the total probable errors ρ of one measured co-ordinate (referred to an arbitrary zero) derived from six images measured in two positions of the plate, and the part of this error, which is inherent in the plate, and not due to the pointings alone (plate error η).

We have the following comparison :—

	f. m	ρ. ″	η. ″
Various Carte du Ciel Instruments (Cape, Helsingfors, Pulkowa)	3·4	±0·055	±0·047
Leiden, 1921	5·2	± ·031	± ·010
Cape, 1913–15, 1924; Greenwich, 1916–19	6·8	± ·032	± ·023
Yale Southern, 1927, 1928	10·9	± ·021	± ·012

There is—if we leave the result for Leiden, which depends on only one series of plates taken by Dr. van den Bos, out of account—a regular decrease of the errors ρ and η with increasing focal length. Whether extrapolation to still larger focal distances is allowed, is doubtful. Probably the errors ρ and η will with increasing focal distance approach asymptotically to a finite limit not very far below the values for $f = 10{\cdot}9$. Moreover, there is a practical limit set by the size of the plate, or rather by the linear distance to be measured corresponding to the angular distance to be determined, which is about 900″ as a maximum. The absolute mean scale value of a series of observations is, of course, determined from the mass of Jupiter, and its effect does not enter into the probable errors ρ and η. But means must be provided on the plates to determine the relative scale value of the plates taken on different nights, by a pair of standard stars, of which the distance should be several times the maximum distance between the satellites. This requires a large field, and for this reason the use of reflectors seems to be excluded.

	″
Gill	± 0·070
Cookson	± 0·100

If on one night all six combinations are measured, the co-ordinates of each of the satellites relative to an arbitrary origin can be derived from these with a weight of 2, if the weight of one measured distance is taken as unity. The time taken by such a complete set was, for Gill's observations, on the average about one hour and a half, including the measures of standard stars, readings of meteorological instruments, etc., and also including the measures of the position angles. The

complete measurement of one plate by an experienced observer, like Mr. Pels at the Leiden Observatory, takes about two hours and a half, including accurate measures of the x co-ordinates only. If also the y co-ordinates are accurately measured, the time required is much longer. If we add the time needed for the taking, developing, etc. of the plate, we can thus say that one plate requires more than twice the observing time * of one complete heliometer night, or, combining with the factor 2 found above, that the weight derived from an equal time of observing is for the heliometer more than four times that for one plate. The above probable errors of one distance must thus be divided by at least 2 to make them comparable with the errors ρ given for the plates, thus giving the upper limits :

$$\begin{array}{rl} \text{Gill} & \rho < \pm\ 0''\!\cdot\!035 \\ \text{Cookson} & < \pm\ \cdot 050 \end{array}$$

The focal distance of the Cape heliometer is 2·5 meters. Comparing with the table given above for photographic telescopes of different focal lengths, it is evident that for equal focal length the heliometer is very much more accurate than the photographic plate.

In the George Darwin lecture of four years ago Professor Schlesinger compared the accuracy of visual and photographic observations generally, and ascribed the superiority of the latter to the fact that *simultaneous* images of the two objects, of which the distance is measured, are used, and the influence of small irregularities of refraction, which can run up to half a second of arc, are thus eliminated, whilst a visual observer, who points the wires of his micrometer first on one star and then on the other, includes in his measure the amount of these fluctuations during the interval. Schlesinger points out that the great accuracy realised by observers of close double stars is due to the fact that they can concentrate their attention on the two stars simultaneously, the images being so close together. Now in the heliometer the images of the two stars are superposed, and the observer with the heliometer enjoys the same freedom from the atmospheric fluctuations as the photographic plate, independently of the distance, so long as this does not become so large that the fluctuations are different for the two objects. Professor Schlesinger gives, however, a diagram of the trails of the stars Merope and Alcyone, showing that the fluctuations were identical for the two stars. The distance between these stars is larger than the largest distance ever occurring in the system of the satellites. On the other hand, as regards systematic errors depending on magnitude (the so-called guiding error), and the like, the heliometer observer has certainly a great advantage over the photographic plate.

The two points on which the representation of the observations by the theory is not entirely satisfactory are the contradiction between the

* It should be mentioned, however, that the further reductions are considerably simpler and more straightforward for the photographic plate than for the heliometer. The total time needed to reach the final results is certainly many times shorter for a series of plates than for an equivalent heliometer series.

results from ancient and modern observations as regards the equation of the centre of Satellite III., and the large residuals in longitude at some epochs of all satellites. As to the first point, it depends on the eclipse observations of the eighteenth century, and can only be entirely settled in the future when a sufficient interval of time shall have elapsed to enable us to determine the secular motions and the ratio of the two eccentricities of III. independently of the old eclipses, *i.e.* after about half a century from the present date. To settle the second point it will, I think, be necessary to observe the satellites as discs, instead of as points of light. To do this photographically is impossible, since, if the focal length were made so large as to show well-defined discs, the distances to be measured, which are of the order of a thousand times the diameters of the discs, would become too large for measurement. Visually a power of about 750 or more would be required.* This would again make the distances too large for micrometer measures. The only solution seems to be a heliometer of large focal length. In accordance with what has just been found regarding the relative accuracy, a focal distance of, say, 5 to 7 metres may be expected to give the same accuracy as the best photographic observations.

By the great magnification of the time scale mentioned in the beginning of this lecture, the interval of modern observations—1891 to 1924—on which the solution of 1928 depends, is equivalent in number of revolutions to 17 centuries of the system of the four inner planets. This corresponds to about the age of pre-telescopic astronomy—from Hipparchus to Tycho. Now to compare the accuracy of the observations of the satellites with those of the inner planets, we must take account of the distance of the observer from the observed body. A fair comparison is reached if we multiply the average value of ρ as found above, say $0''{\cdot}030$, by the ratio of the mean distance of Jupiter to the semi-axis major of Satellite III., say 700. This gives $\pm\ 0'{\cdot}35$, and this may be considered to represent about the accuracy of the best observations made before the invention of the telescope. This comparison would lead us to expect that our present knowledge of the system of the satellites should be about as accurate as the knowledge of the system of the inner planets after Tycho's work had been fully utilized. The following table gives in its upper half the differences of the elements of the four inner planets as derived by Kepler and as given by Newcomb's tables for the epoch 1600, and in its lower half the probable errors of the latest solution of the corresponding elements of the four satellites.

* The power used on the Cape heliometer was 250, and the satellites were seen as points of light, not discs.

Kepler–Newcomb (1600).

	☿.	♀.	♂.	♁.
$\Delta n/n$	3×10^{-6}	10^{-5}	$7/6\times10^{-7}$	$3/2\times10^{-6}$
$2\Delta e$	+0°·634	+0°·028	+0°·116	−0°·044
$2e\Delta\varpi$	+0°·655	−0°·062	−0°·012	+0°·056
Δi	−0°·097	−0°·024		−0°·010
$i\Delta\Omega$	−0°·143	−0°·004		+0°·009
$\Delta e/e$	+ ·027	+ ·035	+ ·057	− ·0042
$\Delta i/i$	− ·014	− ·007		− ·0054
$\Delta\frac{d\varpi}{dt}\Big/\frac{d\varpi}{dt}$	+ ·28	+ ·35	+ ·0033	+ ·010
$\Delta\frac{d\Omega}{dt}\Big/\frac{d\Omega}{dt}$	+ ·50	+ ·33		+ ·332

Satellites 1929 (*p.e.*).

	I.	II.	III.	IV.
$\Delta n/n$	$\pm10^{-9}$	$\pm3/2\times10^{-9}$	$\pm3\times10^{-9}$	$\pm10^{-8}$
$2\Delta\varepsilon$	±0°·0007	±0°·0015	±0°·0025	±0°·0010
$2\varepsilon\Delta\varpi$	±0°·0005	±0°·0012	±0°·0028	±0°·0010
Δi	±0°·0014	±0°·0009	±0°·0010	±0°·0009
$i\Delta\Omega$	±0°·0014	±0°·0009	±0°·0010	±0°·0009
$\Delta e/e$	± ·54	± ·10	± ·016	± ·0012
$\Delta i/i$	± ·044	± ·0019	± ·006	± ·0037
$\Delta\frac{d\varpi}{dt}\Big/\frac{d\varpi}{dt}$	..	± ·037	± ·021	± ·011
$\Delta\frac{d\Omega}{dt}\Big/\frac{d\Omega}{dt}$	± ·0047	± ·0008	± ·012	± ·027

It will be seen that in nearly all cases the elements of the satellites are now far more accurately known than those of the inner planets as determined by Kepler. The explanation of this is, of course, that we had in our investigation the powerful help of the mathematical theory, while Kepler's work was necessarily entirely empirical.

Composite Polytropic Gas Spheres.

By Henry Norris Russell.

(1) *Statement of the Problem.*—In a mass of fluid at rest under its own gravitation, which obeys the polytropic relation $P = k\rho^{1+\frac{1}{n}}$, it is well known that we may set

$$P = k\theta^{n+1}; \quad \rho = \theta^n; \quad \xi = \alpha r; \quad \alpha^2 = \frac{4\pi G}{k(n+1)} \quad . \quad (1)$$

and obtain the differential equation : *

$$\frac{d}{d\xi}\left(\xi^2 \frac{d\theta}{d\xi}\right) + \xi^2\theta^n = 0 \quad . \quad . \quad . \quad (2)$$

Much has been written on this equation.† The solutions are of three types (named by Fowler and Milne) :

(*a*) E-solutions, with θ finite and a maximum at the centre ($\xi = 0$).
(*b*) M-solutions, with θ infinite at the centre.
(*c*) F-solutions, in which $\theta = 0$ for some finite value of ξ, increases to a maximum, and diminishes.

If it is postulated that the actual masses and densities are always finite and positive, only the E-solutions can represent independent masses of fluid throughout their extent. The others may hold for a mass of fluid exterior to a sphere, of suitable mass and radius, which serves as internal support. The internal constitution of this sphere is not directly relevant to the equilibrium outside. When $n > 5$ there are no M-solutions.‡ The E-solutions and some F-solutions give infinite mass and radius, while other F-solutions make these quantities finite. When $n = 5$ there exist M-solutions which, like the E-solutions, have infinite radius but finite mass, while for the F-solutions both are finite. For values of n between 5 and 1 all the solutions have finite mass and radius.§ With given initial values of ξ and θ there are in this case at most two E-solutions.‖ If the initial slope $\frac{d\theta}{d\xi}$ lies between the slopes for these two solutions, we have an M-solution ; if it is outside these limits, an F-solution. With other initial values of ξ and θ the two E-solutions may coincide or disappear, and all other solutions are F-solutions.¶ Little is known of the general solution when $n < 1$.

* The polytropic relation is essentially one between the total pressure and density. Temperature, radiation-pressure, and gas-laws are irrelevant to the problem. Hence θ is to be regarded as a convenient auxiliary variable.

† R. Emden, *Gaskugeln* (Leipzig, 1907), referred to as Emd.; R. H. Fowler, *M.N.*, **91**, 63–91, 1930; E. A. Milne, *M.N.*, **91**, 20–29, 1930.

‡ Emd., p. 219. § *Ibid.*, pp. 207–218. ‖ *Ibid.*, p. 224.

¶ Fowler (*op. cit.*, p. 66) proves the proposition for $n=3$ and $n=3/2$. Emden's discussion shows that it is generally true if $1 < n < 5$.

There are still E-solutions forming a series continuous with those for larger values of n.

The solution of (2) may be expressed in the form

$$\theta\xi^{\frac{2}{n-1}} = F_n(\xi/C) \quad . \quad . \quad . \quad . \quad (3)$$

where C is a constant of integration, and F_n a function depending on n and on another constant of integration, which may be derived from the solution of a certain associated equation of the first order.* This equation expresses the similarity relations by which an infinite number of solutions can be obtained from any known solution of (2) by changes of scale in ξ and θ. The E-solutions have been calculated accurately by quadratures by Emden. His tables† give u (equivalent to our θ) as a function of $\mathfrak{r}\left(\text{equivalent to } \frac{\xi}{C}\right)$ for various values of n. They also give $\frac{\rho}{\rho_c} = u^n$, and $\frac{\rho_c}{\rho_m} = -\mathfrak{r}\Big/\left(3\frac{du}{d\mathfrak{r}}\right)$, where ρ is the density at the distance $\mathfrak{r}$, ρ_c the central density, and ρ_m the mean density of the mass inside radius $\mathfrak{r}$.

(2) *Graphical Criteria for the Type of Solution.*—If we set

$$A = \frac{\rho}{\rho_m} = -\frac{1}{3}\theta^n\xi\frac{d\xi}{d\theta} \quad . \quad . \quad . \quad (4)$$

$$B = (n+1)\xi^2\theta^{n-1} \quad . \quad . \quad . \quad . \quad (5)$$

the values of A and B, for successive points on each E-solution, may be found from Emden's tables. If A and B are plotted as rectangular co-ordinates, we obtain a series of curves En (fig. 1). For values of n between 1 and 5 these curves are simple arcs joining the points A = 1, B = 0, and A = 0, B = 0.

From these curves it is easy to determine the nature of a polytropic solution with any given initial conditions. Let ρ_0 be the actual mean density of the part of the mass inside any given radius r. We have

$$\frac{dP}{dr} = k(n+1)\theta^n\frac{d\theta}{dr} = -g\rho = -\tfrac{4}{3}\pi G r\rho_0\theta^n$$

whence

$$\frac{d\theta}{d\xi} = -\tfrac{1}{3}\alpha r\rho_0 \quad . \quad . \quad . \quad . \quad (6)$$

(a well-known relation).

It follows that we will have an M-solution if, and only if, $\frac{\rho}{\rho_0}$ lies between the two values of $\frac{\rho}{\rho_m}$ corresponding to the E-solutions which

* Emd., pp. 48–60, 201–226.
† *Ibid.*, pp. 76–85.

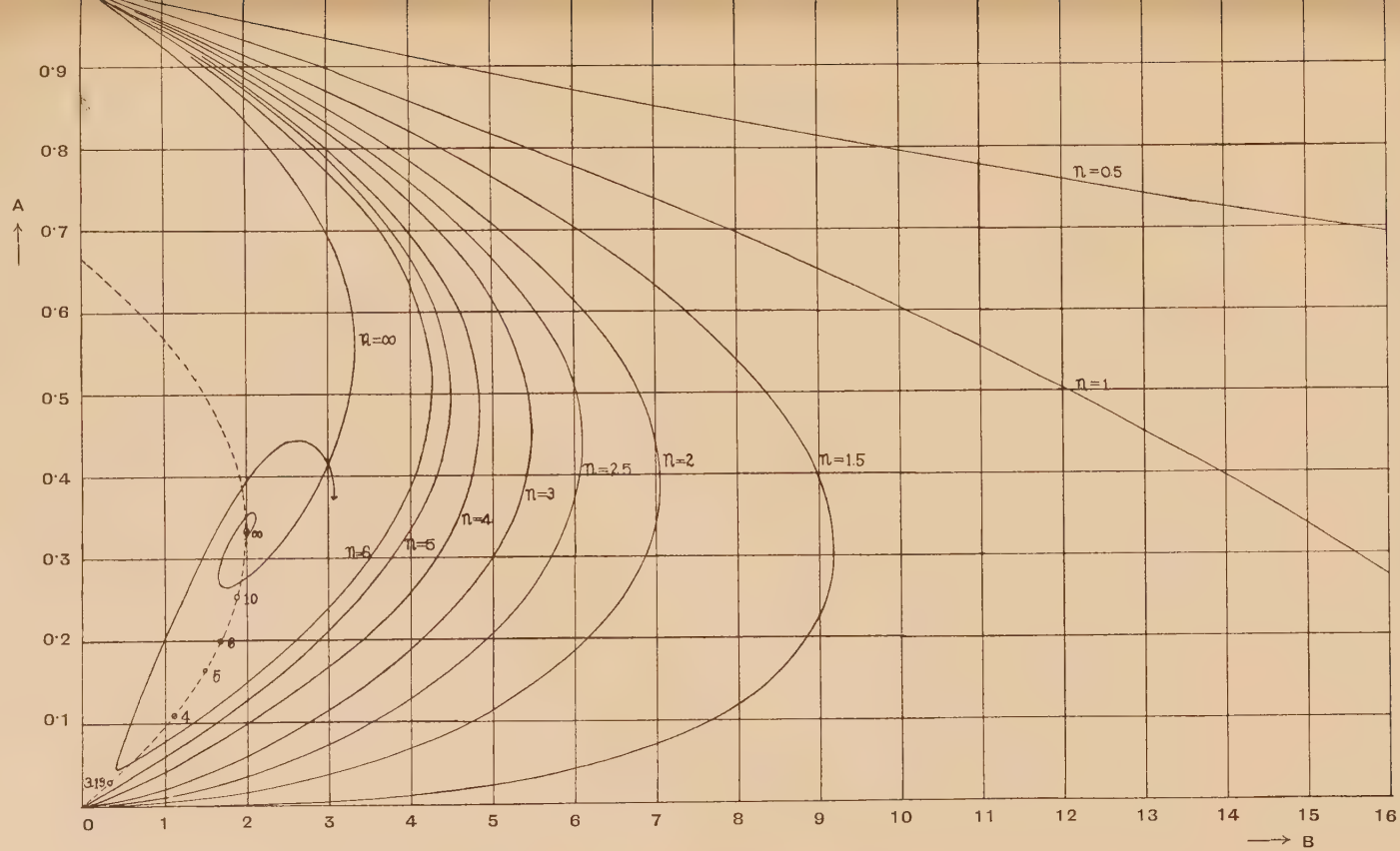
A
B
0·9
0·8
0·7
0·6
0·5
0·4
0·3
0·2
0·1
0
1
2
3
4
5
6
7
8
9
10
11
12
13
14
15
16
n=0·5
n=1
n=1·5
n=2
n=2·5
n=3
n=4
n=5
n=6
n=∞
∞
10
6
5
4
3·19σ

FIG. 1.

satisfy the initial conditions (if such solutions exist). In other words, if we set

$$\left.\begin{aligned} A &= \frac{\rho}{\rho_0} \\ B &= (n+1)\xi^2\theta^{n-1} = \frac{4\pi G}{k}r^2\rho^{\frac{n-1}{n}} = 4\pi G\frac{r^2\rho^2}{P} \end{aligned}\right\} \quad . \quad (7)$$

we will have an M-solution, an E-solution, or an F-solution, according as the point P, which has the co-ordinates A, B, lies inside, upon, or outside the curve En corresponding to the given polytropic index.

For an isolated sphere, of radius r and density ρ, the central pressure is $p' = \frac{2}{3}\pi G\rho^2 r^2$. Hence $B = 6p'/P$. The quantities A and B are dimensionless, and are moreover independent of the constants n and k which define the polytrope. Fig. 1 therefore affords a convenient solution, applicable to all values of n between 1 and 5.

(3) *Composite Polytropic Spheres.*—Suppose now that the mass consists of concentric shells built upon different polytropes. For the central sphere we must have an E-solution. Representing the conditions at any point x in the body by a point P on our diagram, we have $A = 1$, $B = 0$ when x is at the centre. As x moves outward along a radius, P will follow the appropriate En curve until x reaches the boundary of the inner region. If the mass is gaseous (as we will now assume), both P and ρ will be continuous across the boundary, and hence, by (7), A and B will be unaltered. Now, if $1 < n < 5$, En curves for larger values of n lie wholly within those for smaller values. Hence, if at the boundary n increases, the solution in the new shell will always be an F-solution. If n decreases, it will be an M-solution. As x moves out into this shell, P will describe some curve on the diagram (n remaining between 1 and 5). If n has increased, P will be initially, and will remain, outside the new En curve. If at a second boundary n again increases (remaining less than 5), P will *a fortiori* be outside the corresponding En curve, and we will still have an F-solution, and so on, for any number of increases in n, and hence for a continuous increase. Similarly for any number of successive decreases of n, there is always an M-solution in each successive shell (so long as $n > 1$).

If at the surface of the mass we have an M-solution, we can avoid an infinite central density only by passing to a polytrope of *greater* index—that is, if the effective compressibility of the gas increases. Starting with an F-solution, we must meet with a decrease of n and of the compressibility if we are to avoid a "hole" at the centre. Where the change to a specified polytrope should be made can be found from fig. 1. As the solution is continued inward by quadratures, the values of P, ρ, and ρ_m are determined, A and B can be found, and the path of P plotted. When it crosses any En curve the change to the corresponding polytrope is due.

The problem of the composite polytropic sphere thus reduces to a study of the E-curves, and of the F-curves and M-curves for the other solutions.

(4) *Properties of the E-Curves.*—These curves are closely related to a system discussed at length by Emden (pp. 51–56, 201–220). If we set

$$z = \theta\xi^{\frac{2}{n-1}} \qquad y = -\xi\frac{dz}{d\xi} \quad . \quad . \quad . \quad (8)$$

the equation (2) reduces to

$$y\frac{dy}{dz} + \frac{5-n}{n-1}y + \frac{6-2n}{(n-1)^2}z + z^n = 0 \quad . \quad . \quad (9)$$

From (8) we have

$$-\frac{y}{z} = \frac{2}{n-1} + \frac{\xi}{\theta}\frac{d\theta}{d\xi}$$

Then, by (4) and (5), which apply to all solutions of (2),

$$\left.\begin{aligned} B &= (n+1)z^{n-1} \\ 3(n+1)A &= Bz\Big/\left(y + \frac{2}{n-1}z\right) \end{aligned}\right\} \quad . \quad . \quad . \quad (10)$$

Emden gives in detail the (y, z) curves corresponding to the E-solutions. When n lies between 1 and 5 these curves start from points (corresponding to the surface of the star) at which $z = 0$, $y = y_1 = -\xi_1^{\frac{n+1}{n-1}}\left(\frac{d\theta}{d\xi}\right)_1$ (where ξ_1 is the outer radius). The values of this quantity are

$n =$	1	1·5	2	2·5	3	4	4·5	5
y_1	∞	132·8	10·50	3·87	2·02	0·73	0·39	0
$y_1(n+1)^{\frac{n}{n-1}}$	∞	448	42·0	17·9	10·5	4·7	2·7	0

We have then, when x and y are small,

$$A = \frac{z^n}{3y_1} = \frac{1}{3y_1(n+1)^{\frac{n}{n-1}}}B^{\frac{n}{n-1}} \quad . \quad . \quad . \quad (11)$$

For a fixed value of B, A increases steadily with n, so that the curves with greater n lie inside those with smaller n, right up to the origin, at which they all touch one another.

To the centre of the sphere corresponds $y = 0, z = 0$. Emden's curve approaches this point so that $\frac{dy}{dz} = -\frac{2}{n-1}$. Here $B \to 0$, but the expression (10) for A is indeterminate. From (2), however, with the adopted initial value $\theta = 1$, we find

$$\theta = 1 - \tfrac{1}{6}\xi^2 + \frac{n}{120}\xi^4 - \ldots$$

whence

$$\left.\begin{aligned} A &= 1 - \frac{n}{15}\xi^2 + \ldots \\ B &= (n+1)\xi^2 + \ldots \end{aligned}\right\} \quad . \quad . \quad . \quad (12)$$

$$B = 15\left(1 + \frac{1}{n}\right)(1 - A) + \ldots$$

so that here again the curves for smaller n are outside those for greater. The curves intersect at the point A = 1, B = 0 (corresponding to the centre of the sphere).

In three cases there are closed expressions for A and B.

(*a*) $n = 0$; A = 1; B indeterminate, giving the straight line E0.

(*b*) $n = 1$; $\theta = \frac{\sin \xi}{\xi}$; $A = \xi^2/3(1 - \xi \cot \xi)$; $B = 2\xi^2$ (13)

giving the transcendental curve E1.

(*c*) $n = 5$; $\theta = (1 + \frac{1}{3}\xi^2)^{-\frac{1}{2}}$; $A = (1 + \frac{1}{3}\xi^2)^{-1}$; $B = 6\xi^2(1 + \frac{1}{3}\xi^2)^{-2}$

giving the parabola E5,

$$B = 18A - 18A^2. \quad . \quad . \quad . \quad (14)$$

When n lies between 0 and 1, P starts from the origin as x starts from the centre, according to (12), but at the surface, as A → 0, B becomes infinite. Near the surface ($\xi = \xi_1$) we may treat $\frac{d\theta}{d\xi}$ as constant. At the depth $e\xi_1$ we have $\theta = -e\xi_1\left(\frac{d\theta}{d\xi}\right)_1$, and

$$A = \tfrac{1}{3}\xi_1^{n+1}\left(\frac{d\theta}{d\xi}\right)_1^{n-1} e^n = \tfrac{1}{3}Ce^n, \qquad B = (n+1)Ce^{n-1}$$

whence

$$B = 3(n+1)\left(\frac{C}{3}\right)^{\frac{1}{n}} A^{1-\frac{1}{n}}.$$

From Emden's data we find

n =	0·5	1·0	1·5	2·0
$(n+1)\left(\frac{C}{3}\right)^{\frac{1}{n}}$ =	6·98	6·58	6·12	5·61

For small values of A it is evident that B will increase as n diminishes. Hence the curves are arranged in the same order as near the origin, and it is proved later (footnote) no two of them intersect.

When $n > 5$ the course of the curves is quite different. In the y, z plane they start from the origin, and wind in spirals about the point $y = 0$, $z = \left(\frac{2n-6}{(n-1)^2}\right)^{\frac{1}{n-1}}$, approaching it asymptotically so that the distance from the limit diminishes approximately in the ratio

$$exp\{-\pi(n-5)(7n^2 - 22n - 1)^{-\frac{1}{2}}\}$$

after each revolution.* This limit-point represents a singular solution of (2), which may be written

$$\xi^2\theta^{n-1} = \frac{2n-6}{(n-1)^2}.$$

* Emden, p. 153.

In the A, B plane the En curves start from A = 1, B = 0 according to (12), but turn back before reaching the origin, and wind, in distorted spirals, about the points

$$A = \frac{n-3}{3(n-1)} \qquad B = \frac{2(n+1)(n-3)}{(n-1)^2} \quad . \quad . \quad (15)$$

which represents the singular solution. For all values of n these points lie on the parabola

$$B = 12A - 18A^2 \quad . \quad . \quad . \quad . \quad (16)$$

The points for $n = 4, 5, 6, 10, \infty$, are plotted in fig. 1. For the isothermal case $n = \infty$, equation (5) for B fails, but (7) may be used. This curve has been plotted from Emden's data (p. 135).

For these spirals there is no longer an inside or outside, which agrees with the fact that there are no M-solutions of (2). The En curves for different values of n usually intersect, so that in this case, and this alone, it is possible to pass continuously from one E-solution to another with different n. The "rate of convergence" of the spiral becomes small when $n - 5$ is small, so that for n very slightly exceeding 5, the curve will be close to the parabola E5, almost as far as the origin, then return towards its starting-point, close to the axis B = 0, and sweep round in slowly closing coils.

The inner parts of the spirals represent the outer portions of spheres of infinite radius and mass. To be physically significant these curves must be described in the opposite direction, representing the inner portions of spheres for which the outer parts belong to M-solutions with smaller index.

The values of A and B may be given, and it may be asked what values of n will lead to an E-solution. We may limit ourselves to cases where $A < 1$ (since for $A > 1$ the density must decrease inwards somewhere in the central region). Two cases arise.

(*a*) P lies outside the parabola E5. In this case there is one solution, and only one, as is evident from fig. 1; and for this $n < 5$.

(*b*) P lies inside E5. We must have $n > 5$, and the E-curves are of spiral form. When $n - 5$ is very small, the curve consists of closely-wound convolutions, and a large number of them will pass between P and the limits of the area. As n increases, these coils will shrink and one after another of them will pass through P. Hence there will in general be many solutions. It may be suspected that the number is infinite, with successive values of n approaching 5 as a limit, but to investigate this would demand a study of the form of the spiral when $n - 5$ is very small, which will not be attempted here.

(5) *The F-solution and Curves.*—When $n < 5$ all the F-solutions have finite radii; when $n > 5$ some of them have and others have not. The latter, possessing a central "hole" and infinite radius and total mass, need not concern us here.

With a finite boundary, whatever the value of ξ, we start with A = 0, B = 0, or, in the y, z plane, with $z = 0$, $y_0 > y_1$. Near the

origin we have approximately

$$A = \frac{1}{3y_0(n+1)^{\frac{n}{n-1}}} B^{\frac{n}{n-1}},$$

so that the curve starts with the same shape as the En curve, but lies outside it. At the very beginning it lies inside any E-curve with a smaller n. Emden has proved that all the corresponding curves in the y, z plane reach a maximum z when $y = 0$, and afterwards cut the line $y + \frac{2}{n-1}z = 0$ for some positive value of z. The Fn curves therefore reach a maximum value of B, and then pass away toward $A = +\infty$ for some positive value of B. Hence every Fn curve which starts at the origin with $n \leqslant 5$ must cross all the En curves for smaller values of n, as far as $n = 0$ (beyond which nothing is known about their form). When $n > 5$ the initial arc of the F-curve lies outside the E-curve for $n = 5$ (for which, near the origin, $A = \frac{1}{18}B$), but inside all those for smaller n.

It may be proved that a given F-curve can cross each of these E-curves but once. To show this, set

$$x = y + \frac{2}{n-1}z \quad . \quad . \quad . \quad . \quad (17)$$

Then (10) becomes

$$A = \frac{z^n}{3x} \qquad B = (n+1)z^{n-1} \quad . \quad . \quad . \quad (18)$$

and (9) reduces to

$$y\frac{dx}{dz} + x\left(\frac{3-n}{n-1} + 3A\right) = 0.$$

Differentiating (18) logarithmically we find

$$(n-1)\frac{B}{A}\frac{dA}{dB} = n - \frac{z}{x}\frac{dx}{dz} = n + \frac{z}{y}\left(\frac{3-n}{n-1} + 3A\right).$$

But by (17) and (18)

$$\frac{z}{y} = \frac{3(n^2-1)A}{(n-1)B - 6(n+1)A}$$

giving after reduction

$$\frac{B}{A}\frac{dA}{dB} = \frac{nB - 9(n+1)(A - A^2)}{(n-1)B - 6(n+1)A} \quad . \quad . \quad (19)$$

This equation is a transformation of Emden's equation (9) in which n no longer appears as an exponent, but only as a coefficient. Enough is known, however, about the character of the solutions of (9) to make it unprofitable to attempt to integrate (19) in finite terms.

The points at which $\frac{dB}{dA} = 0$ lie on the straight line

$$(n-1)B = 6(n+1)A \quad . \quad . \quad . \quad (20)$$

(corresponding to Emden's $y = 0$); and those for which $\frac{dA}{dB} = 0$, on the parabola

$$nB = 9(n+1)(A - A^2) \quad . \quad . \quad . \quad (21)$$

These curves intersect at the origin (corresponding to the surface) and at $A = \frac{n-3}{3(n-1)}$, $B = \frac{2(n+1)(n-3)}{(n-1)^2}$, corresponding to the singular solution. At these points the value of $\frac{dB}{dA}$ becomes indeterminate. The points at which the tangent to the curve passes through the origin lie on the parabola

$$B = 3(n+1)(A - 3A^2) \quad . \quad . \quad . \quad (22)$$

which also passes through the singular point. For points inside this parabola the curves for index n are convex towards the origin; elsewhere they are concave towards it (except for portions of the curves when $n < 1$). For all values of n greater than 1, the curve (21) lies inside the parabola E5. So long as any solution remains outside this, $\frac{dA}{dB}$ cannot vanish; if the curve starts with a positive value it must retain one.

Now, from (19) we find

$$\frac{B}{A}\frac{dA}{dB} = \frac{B - 9A + 9A^2}{B - 6A} + \frac{B(B - 12A + 18A^2)}{(B - 6A)^2 n - (B^2 - 36A^2)} \quad . \quad (23)$$

It follows from this that, for fixed values of A and B, $\frac{dA}{dB}$ decreases algebraically, and $\frac{dB}{dA}$ increases, as n increases, so long as $B - 12A + 18A^2 > 0$—that is, so long as P lies outside the parabola (16) which passes through the singular points. For points on this parabola all solutions of (19) have a common tangent. It is a tac-locus. At any intersection of two curves (whether belonging to E-, F-, or M-solutions) which lies outside the tac-locus, the curve with greater n passes to the side of greater B as A increases. The reverse is the case inside the tac-locus.

Every F-curve starting from the origin must have $n > 1$. If $n < 5$, the curve certainly lies in the region where A increases monotonically. At every intersection of such a curve with an E-curve, n is greater for the former. Hence the F-curve passes outside the E-curve, and it can intersect a given one but once.*

* This is true for any pair of curves, whether of types E, F, or M, unless one at least of them passes inside the tac-locus. Hence no two En curves with $0 < n < 5$ can intersect more than once. But it has already been proved that when A is small, and also when $1 - A$ is small, these curves are arranged in order of increasing n. Hence no two of them intersect at all except at the origin and at $A = 1$, $B = 0$. This affords formal proof of the proposition previously established graphically.

If $n > 5$, an F-curve starts from the origin outside the curve E5 and inside all those for smaller n. At any small finite distance from the origin it will intersect some E-curve with n slightly less than 5, and must pass outside this. Hence it always remains outside E5, and will cross all the E-curves with $n < 5$.

The E-curves for $n > 5$ lie entirely inside of E5, and hence cannot intersect F-curves starting from the origin and corresponding to finite spheres, though they will, in general, intersect F-curves corresponding to spheres of infinite radius and mass. It follows that a gas-sphere in which the outer regions follow an F-solution with $n_1 > 1$ can be "finished off" to a finite central density by transition to another polytrope of given index n in one and only one way, so long as n lies between n_1 and zero (or between 5 and 0, if $n_1 > 5$).

(6) *The M-solutions and Curves.*—These solutions exist only when $n < 5$. The curves start inside the corresponding E-curves and remain within them, and the analysis just given shows that each curve can cut the E-curves for larger values of n—up to and including 5—once at most. To prove that it actually does so, we must consider the limit of the M-solution at the centre.

When $1 < n < 3$ Emden has shown (p. 204) that, as $\xi \to 0$, the y, z curve corresponding to an M-solution tends toward the origin in such a way that $y + \frac{3-n}{n-1}z = 0$, or by (17) $x = z$. By (18) A and B both tend to zero in such a way that $B = 3(n+1)A$. The M-curves therefore return to the origin in such a direction that they lie inside the curve E5 and the tac-locus. These solutions have a point-mass at the centre.

When $3 < n < 5$ Emden proves (pp. 213–216) that the y, z curves end at the points corresponding to the singular solution. The A, B curves will therefore approach the points (15) as limits. When $n > 3{\cdot}1876$ the curves wind about these points in spirals; otherwise they go directly into them. The limits are on the tac-locus and well inside E5. There is in this case no central point-mass.

When $n = 3$ Milne has shown* that, if ξ is small, we have approximately

$$\theta = \sqrt{\tfrac{1}{2}}\xi^{-1}(\log D/\xi)^{-\frac{1}{2}}$$

where D is an arbitrary constant. In this case $\rho_m = 6\rho \log D/\xi$

$$B = \frac{2}{\log D/\xi} = 12A.$$

There is no point-mass, and the M-curve approaches the origin inside E5 and touches the tac-locus.

Hence, in all cases, the M-curve passes inside E5. A gas sphere which starts at the surface with an M-solution when $n_1 > 1$ can therefore be "finished off" in one, and only one, way by transition to an E-solution with a given index between n_1 and 5 (inclusive).

When $n > 5$ for the central E-solution the situation is much more

* *M.N.*, **91**, 23, 1930.

complicated, on account of the spiral form of the E-curves. If $1 < n_1 < 3$ the M-curves form loops, starting from the origin with $\frac{dA}{dB} = 0$, and returning with $\frac{dA}{dB} = \frac{1}{3(n+1)}$. If at the surface $\frac{d\theta}{d\xi}$ is but little less than for the E-solution, $y_0 - y_1$ will be small, the curve Mn will run close to En, almost up to the point A $= 1$, B $= 0$, and then return to the origin near the axis B $= 0$. Such a curve will enclose the limit-points of all the spirals, and cut them all an odd number of times. For the wider spirals the number of intersections will be 3, 5, 7 . . . probably increasing without limit as $n \to 5$.

As y_0 increases, the M-loop contracts. The number of intersections with any given spiral will at first increase as more of the convolutions are met, becoming infinite when the loop passes through the limit-point of the spiral, and then diminishing, being now even instead of odd. Finally, when the loop shrinks so far as to touch externally the outermost convolution of the spiral (at its intersection with the tac-locus), the last pair of real intersections disappears. The figure suggests that this happens first for $n = \infty$, and then successively for smaller values.

The loop finally shrinks to indefinitely small size. Near the origin, neglecting A^2, and setting $\frac{A}{B} = t$ in (19), we have

$$B\frac{dt}{dB} = \frac{t\{1 - 3(n+1)t\}}{n - 1 - 6(n+1)t}$$

with the integral

$$B = Ct^{n+1}\{1 - 3(n+1)t\}^{3-n}.$$

Along the tac-locus $t = 12$, and the loop touches the spiral when

$$B = 12^{n-1}\left(\frac{3-n}{4}\right)^{3-n} C.$$

The maximum value of this coefficient is $\frac{1}{4}$, when $n = 1$. However small the loop may be, it will cut all the E-curves up to $n = 5$, but the spirals with $n > 5$ will intersect it only when $n - 5$ is very small. M-solutions of this type correspond to very small initial values of $\frac{d\theta}{d\xi}$. If $n < 3$ they represent cases where most of the force of gravity must be ascribed to a point-mass at the centre. For $n = 3$ there is no point-mass, but the central condensation is very great. To "finish off" such solutions with an E-solution demands a great central condensation in this, which ceases to be possible when n is but a little greater than 5.

When $3 < n_1 < 5$, if $y_1 - y_0$ is small, the M-curve again lies close to the En curve almost up to A $= 1$, and then returns near the axis, passing finally to the singular point. In such a case it will cut all the E-curves up to $n = \infty$. When $n < 3{\cdot}188$ the M-curve passes directly to the limit-point, and the relations closely resemble those just discussed, except that there will always be at least one intersection with any E-curve whose first convolution passes outside the singular point, so

that the limiting value of n, below which there is always a solution, will exceed 5.

When $n > 3{\cdot}188$ the M-curve approaches its limit along a spiral, winding in the opposite sense from the E-curves for $n > 5$, and converging very slowly if n is near 5, but very rapidly if n is near 3·19. For a fixed value of n_1 there will still be intersections with all the E-curves up to $n = \infty$ when $y_1 - y_0$ is small. As y_0 diminishes, the number of intersections with a given En curve will become infinite whenever a convolution of the M-spiral passes through the limit-point of the E-spiral. If the limit-point of the M-spiral lies inside the outermost convolution of the other, the two will intersect for all values of y_0; if not, they may fail to do so. Detailed curve-tracing would be required to determine under what conditions this happens, but the figure indicates strongly that it will occur if n is large and n_1 near 3·19.

This completes the discussion of spheres made up of two polytropic shells. When there are three or more shells, the conditions at the surface, and at all the transitions except the last, may be assigned arbitrarily, and the last transition discussed as above. The only important difference is that, when $n > 5$, a class of F-solutions which for a complete sphere give infinite radius and mass with a "hole" in the middle, comes into consideration, and also a class of solutions with $n = 5$, giving infinite central density, infinite radius, and finite mass. Such solutions can occur only in the intermediate layers of a physically plausible sphere.

(7) *Summary.*—(i) A graphical method is developed for determining the nature of the solution of the equation of polytropic equilibrium in a spherical mass of gas, given the pressure and density at a given distance from the centre and the whole mass inside a sphere of this radius. The result is obtained by plotting on the diagram illustrated in fig. 1 a point whose co-ordinates are given by (7) and depend on the constants specified above, but not on those of the polytrope.

(ii) If P lies outside the curve marked E5 in the figure, the solution will be an E-solution, an M-solution, or an F-solution (with finite density, infinite density, or a "hole" at the centre) according as P lies upon, inside, or outside the E-curve for the given polytropic index n. There is one, and only one, value of n for which an E-solution exists which satisfies the four initial conditions, and this value lies between 1 and 5. For larger values of n we get an F-solution, for smaller an M-solution.

(iii) If P lies inside the curve E5 there will always be at least one E-solution, and the number is probably infinite. For all these $n > 5$, and for most of them $n - 5$ is very small. All these solutions correspond to spheres with finite central density, but infinite radius and mass. For other values of n, the solution is of the F-type. With such initial conditions no configuration built upon a single polytrope is physically possible.

(iv) For spheres with the inner portion built upon one polytrope and the outer part on another, there must be an E-solution in the inner region. In the outer region we find an M-solution if n is less than in

the interior, and an F-solution if it is greater. If there are several outer shells, each with its own index, and n always diminishes outward, there are M-solutions in all the shells; and E-solutions if n always increases. If n diminishes or increases continuously, and later becomes constant, there is an M-solution or E-solution respectively in the region outside.

(v) For a sphere of given mass and radius, and an F-solution in the outer region, with $n_1 > 1$, there is one, and only one, way in which the solution can be completed by transition to an E-type with given n. If $n_1 < 5$ we must have $n_1 > n > 0$; if $n_1 > 5$, $5 > n > 0$. Transitions to E-solutions with $n > 5$ are impossible.

(vi) If there is an M-solution in the outer region, there is one, and only one, way of completing the solution by transition to an E-solution, with n lying between n_1 and 5. The transition to an E-solution with a given value of n exceeding 5 is usually, but not always, possible. This change can often be made in two or more ways, and, for certain values of n, the number of ways may become infinite.

In conclusion, it should be stated that, although the results of this paper indicate that Milne's "condensed" stars, with very dense cores and extensive envelopes, will not occur under his very specialised and greatly simplified assumptions, it does not follow that such stars may not occur in nature. The writer hopes to return to other aspects of the problem in the near future.

Princeton University Observatory:
1931 *April* 21.

By the courtesy of Professor Milne I have seen a proof of his note (which follows this paper). The conditions which he assumes are different from those postulated above, and the results naturally differ. It is gratifying to have so quick a confirmation of the opinion that "condensed" masses may occur under different formal assumptions, and interesting to see how small the necessary changes in these are. I prefer at present to suspend judgment on the question whether they actually occur in nature.

Princeton: 1931 *May* 27.

Note on Steady-state Distributions which are given by Solutions of Emden's Differential Equation. By E. A. Milne.

Professor H. N. Russell's valuable paper (p. 739) on composite polytropic gas spheres is closely related to earlier papers by Cowling * and Strömgren,* though written entirely independently. It is in its results, though not in method, akin also to a recent paper under a similar title by Eddington.† In view of the amount of attention which

* *M.N.*, **91**, 466, 472, 1931. † *M.N.*, **91**, 440, 1931.

the subject is attracting the following remarks may be of interest. They relate to actual problems which have occurred in unpublished investigations of my own, in which the solution ultimately depends on a differential equation of Emden's type.

Russell defines a polytropic distribution, or, in the case of a composite sphere, a polytropic distribution in a given concentric shell, as one in which the relation between total pressure P and density ρ takes the form

$$\mathrm{P} = k\rho^{1+\frac{1}{n}} \quad . \quad . \quad . \quad . \quad (1)$$

where k and n are constants. In that case, by taking new variables ξ and θ proportional to r and $\rho^{1/n}$, we can reduce the equation of equilibrium,

$$\frac{d\mathrm{P}}{dr} = -\frac{\mathrm{GM}(r)}{r^2}\rho \quad . \quad . \quad . \quad . \quad (2)$$

to Emden's differential equation,

$$\frac{1}{\xi^2}\frac{d}{d\xi}\left(\xi^2\frac{d\theta}{d\xi}\right) = -\theta^n \quad . \quad . \quad . \quad (3)$$

Russell remarks : " The polytropic relation is essentially one between the total pressure and density. Temperature, radiation-pressure and gas-laws are irrelevant to the problem."

The main object of the present note is to point out that Emden's differential equation is of far wider application than to distributions satisfying (1), and that Russell's main theorem does not necessarily apply to these more general polytropic distributions. There are a number of types of steady-state distributions in which the density is represented by the appropriate solution of (3), although no relation of the type (1) exists. I have encountered two such types : (*a*) steady-state distributions in which the relation between P and ρ cannot be integrated ; (*b*) steady-state distributions in which the relation between P and ρ is of the form

$$\mathrm{P} = k\rho^{1+\frac{1}{n}} + \mathrm{D} \quad . \quad . \quad . \quad . \quad (4)$$

where D is a constant not equal to zero, taking different constant values in different zones. At an interface where k and n change discontinuously, though P and ρ are continuous, D is discontinuous.

The following are examples :—

(*a*) Consider the outer portions of a degenerate star in which the luminosity is generated in a core or central region. Then the outer portions are " source-free " and here $\mathrm{L}(r)$ = constant = L. The equations expressing the steady state, in their usual form, are

$$\frac{dp}{dr} + \frac{dp'}{dr} = -\frac{\mathrm{GM}(r)}{r^2}\rho \quad . \quad . \quad . \quad (5)$$

$$\frac{dp'}{dr} = -\frac{\kappa \mathrm{L}}{4\pi c r^2}\rho \, . \quad . \quad . \quad . \quad (6)$$

Eliminating dp'/dr we have

$$\frac{dp}{dr} = -\frac{GM(r)}{r^2}\rho + \frac{\kappa L}{4\pi c r^2}\rho \quad . \quad . \quad . \quad (7)$$

If κ is constant, this yields

$$\frac{d}{dr}\left(\frac{r^2}{\rho}\frac{dp}{dr}\right) = -4\pi G r^2 \rho \quad . \quad . \quad . \quad (8)$$

If the gas-pressure p is related to the density ρ by the equation of state

$$p = K\rho^{1+\frac{1}{n}} \quad . \quad . \quad . \quad . \quad (1')$$

(8) reduces to Emden's equation. But there exists no integrated relation of the type (1) between $P(= p + p')$ and ρ. It is the form of the gas-law, not any (P, ρ) relation, which determines in this case the reduction to Emden's form. But now the remaining mass $M(r)$ is given by

$$M(r) = \frac{\kappa L}{4\pi c G} - \frac{r^2}{G\rho}\frac{dp}{dr},$$

and so for the remaining mean density ρ_0 we have

$$\rho_0 = \frac{M(r)}{\frac{4}{3}\pi r^3} = \frac{3}{4\pi}\frac{\kappa L}{4\pi c G r^3} - \frac{3}{4\pi G}\frac{1}{r\rho}\frac{dp}{dr}.$$

If then

$$\rho = \theta^n, \qquad p = K\theta^{n+1}, \qquad \alpha r = \xi, \qquad \alpha^2 = \frac{4\pi G}{K(n+1)},$$

we have

$$\rho_0 = \frac{3}{4\pi}\frac{\kappa L}{4\pi c G r^3} - \frac{3}{\xi}\frac{d\theta}{d\xi},$$

and so with Russell's definition of A,

$$A = -\tfrac{1}{3}\theta^n \xi \frac{d\xi}{d\theta} = \frac{\rho}{\rho_0 - \frac{3}{4\pi}\frac{\kappa L}{4\pi c G r^3}} \quad . \quad . \quad (9)$$

Now the reduction of (5) and (6) *via* (7) and (8) to Emden's form (3) is entirely independent of the particular values of the constants K and κ. At an interface between two consecutive concentric zones, K and κ will in general change discontinuously, though the new zone still contains a distribution associated with Emden's equation. Thus at an interface A may change discontinuously, the point (A, B) in Russell's fig. 1 will take a leap and his proof no longer applies. Russell's B remains continuous, though in his equation (7) P must now be replaced by p.

(*b*) A degenerate gas has almost certainly a much smaller * opacity

* I believe this remark was first made to me by Professor Lindemann.

than a classical Maxwellian gas. It is therefore desirable to consider models in which the absorption coefficient changes discontinuously as we pass from the classical zone to the degenerate core. We do not thereby assume an actual discontinuity in a star ; we merely recognise that the mean value of the opacity appropriate to the new zone differs from that appropriate to the previous zone. More generally we may consider a "generalised standard model" in which ϵ, the source-strength, is everywhere the same, and the opacity κ is constant in each of a number of zones but takes different values in different zones. By taking a sufficient number of sufficiently thin zones we can represent any opacity distribution.

Then (5) holds as above, but instead of (6), since now

$$\mathrm{L}(r) = \frac{\mathrm{L}}{\mathrm{M}}\mathrm{M}(r),$$

we have

$$\frac{dp'}{dr} = -\frac{\kappa_n \mathrm{L}}{4\pi c\mathrm{M}}\frac{\mathrm{M}(r)}{r^2}\rho \quad . \quad . \quad . \quad (6')$$

in the nth zone. By division of (5) by (6′) we have in this zone

$$1 + \frac{dp}{dp'} = \frac{4\pi c\mathrm{GM}}{\kappa_n \mathrm{L}},$$

whence

$$p = p'\left(\frac{4\pi c\mathrm{GM}}{\kappa_n \mathrm{L}} - 1\right) + \mathrm{D}_n \quad . \quad . \quad . \quad (10)$$

and so

$$\mathrm{P} = p + p' = p\frac{4\pi c\mathrm{GM}}{4\pi c\mathrm{GM} - \kappa_n \mathrm{L}} - \mathrm{D}_n\frac{\kappa_n \mathrm{L}}{4\pi c\mathrm{GM} - \kappa_n \mathrm{L}} \quad . \quad (11)$$

where D_n is constant (in the nth zone). Since p and p' are continuous but κ_n discontinuous, D_n takes different values in the different zones. If the boundary condition in the outermost ($n = 1$) zone is $p = 0$, $p' = 0$, then D_1 is zero, but all the other D_n's are non-zero. But the existence of the integral (11) is entirely irrelevant to the question of whether the distribution in the nth zone can be reduced to Emden's equation. For from (5) and (6′), by elimination of dp'/dr, we have

$$\frac{dp}{dr} = -\left(\mathrm{G} - \frac{\kappa_n \mathrm{L}}{4\pi c\mathrm{M}}\right)\frac{\mathrm{M}(r)}{r^2}\rho \quad . \quad . \quad . \quad (7')$$

whence

$$\frac{d}{dr}\left(\frac{r^2}{\rho}\frac{dp}{dr}\right) = -\left(\mathrm{G} - \frac{\kappa_n \mathrm{L}}{4\pi c\mathrm{M}}\right)4\pi r^2\rho \quad . \quad . \quad (8')$$

This reduces to Emden's equation provided the gas-pressure p is connected with the density ρ by an equation of the type (1′). As Russell says, temperature is irrelevant, but in these two examples total pressure P is equally irrelevant. It is the form of the gas equation which determines the reduction to Emden's equation.

We have now that Russell's A is continuous, but since the substitutions reducing (8′) to Emden's equation are

$$\rho = \theta^n, \qquad \alpha r = \xi, \qquad \alpha^2 \mathrm{K}(n+1) = 4\pi\Big(\mathrm{G} - \frac{\kappa_n \mathrm{L}}{4\pi c \mathrm{M}}\Big),$$

we find for Russell's B,

$$\mathrm{B} = (n+1)\xi^2\theta^{n-1} = 4\pi\Big(\mathrm{G} - \frac{\kappa_n \mathrm{L}}{4\pi c \mathrm{M}}\Big)\frac{r^2\rho^2}{p}.$$

Since p, ρ, and r are continuous at an interface but κ_n changes discontinuously, B changes discontinuously. Thus again the point (A, B) in Russell's fig. 1 takes a leap, and his proof no longer applies.

It may be noted that in both these examples we dispense with the consideration, in effect, of the total pressure P. This arises from the circumstance that equations (7) and (7′) are more fundamental than either of the pairs of equations (5) and (6) or (5) and (6′) from which I derived them. Equations (5) and (6) are *approximations*; in general the system of stresses caused by the flow of radiation does not reduce to a pressure p', but is representable only by a tensor $p'_{\alpha\beta}$. But (7) or (7′) is *exact*. The term $\kappa \mathrm{L}(r)\rho/4\pi c r^2$ accurately * gives the mechanical force arising from the flux $\mathrm{L}(r)/4\pi r^2$, whatever the radiation field. The whole analysis is an exact representation of the mechanical equilibrium. But the mention of total pressure is both irrelevant and inexact.

In the case of the generalised standard model, the gas-law $p = \mathrm{K}\rho^n$ always gives a reduction to Emden's equation, but the classical gas-law $p = (\mathrm{R}/\mu)\rho\mathrm{T}$ only gives a reduction to Emden's equation when we employ the P-integral and take $\mathrm{D}_n = 0$, *i.e.* in the outermost zone. I have in this way found it possible to treat fully the composite model consisting of a classical gas-zone of opacity κ_1 enclosing any number of degenerate zones of opacities $\kappa_2, \kappa_3 \ldots$. The set of pairs of equations of fit is easy to derive. This solution is for the uniform source-distribution $\epsilon = \text{const.}$ "Source-free" or "point-source" distributions for degenerate material are equally represented by Emden's equation; but source-free perfect-gas zones require the introduction of a new set of functions, the functions f_1, f_2, f_3 of Section A of my paper "The Analysis of Stellar Structure." † It is, however, easily practicable to treat the composite model consisting of a "source-free" gas-zone of opacity κ_1, surrounding any number of "source-free" degenerate zones of opacities $\kappa_2, \kappa_3 \ldots \kappa_n$, surrounding in turn any number of "ϵ = const." degenerate zones of opacities $\kappa_{n+1} \ldots \kappa_m$.

It may be noted that the source-distribution

$$\frac{\mathrm{L}(r)}{\mathrm{L}} = 1 - a + a\frac{\mathrm{M}(r)}{\mathrm{M}} \qquad (0 \leqslant a \leqslant 1)$$

gives density-distributions which in certain cases reduce to Emden's differential equation; $a = 0$ corresponds to the "source-free" case, $a = 1$ to the "ϵ = constant" case, and any value of a between 0 and 1 to an intermediate degree of source-concentration.

* See *Handbuch der Astrophysik*, Bd. III/1, chap. 2, § 11, pp. 100, 101.

† *M.N.*, **91**, 4, 1930.

Russell's theorem that n must decrease inwards if an M-solution in the outer layers is to surround an E-solution in the core no longer holds for the more general polytropic distributions which are the subject of this note. It is clear on physical grounds that by sufficiently diminishing the opacity of the core we may be able to compress the material sufficiently to fit an "$n = \frac{3}{2}$" E-core inside an "$n = 3$" M-envelope; and I have in fact, from the curves now at my disposal (calculated by Fairclough) actually constructed such solutions.

I desire to express my appreciation of Professor Russell's great courtesy in sending me a separate copy of his paper and diagram before publication.

Further Remarks on Stellar Rotation. By E. A. Kreiken.

§ 1. *Introduction.*—In some recent papers * evidence was given that the period of axial rotation of a star is closely correlated with its diameter. From the stars in Elvey's list † the mean relation

$$\log P = 1{\cdot}49 \log D - k \quad . \quad . \quad . \quad (1)$$

was obtained. In order to check this formula it was applied to the eclipsing variables and spectroscopic binaries with small orbital eccentricity e. According to theory, their periods of axial rotation must be almost identical with those of orbital revolution, so (1) ought to be valid for the latter periods. On the other hand, large positive excesses of the observed periods over the computed ones are to be expected if (1) is applied to the binaries with large eccentricities. From our different diagrams it appeared that the general distribution of the values O − C agreed with expectation. Finally some information as to the mutual relations of the different groups of stars was obtained by applying the equation to the different variables, which are generally believed to indicate a state preceding that of the binaries. Only the statistical side of the question has so far been investigated. Without any doubt it is of great importance to know whether a relation giving the period of axial rotation of a star can theoretically be derived, and to see how this theoretical relation compares with (1). This question is considered in the present paper. The result is that an equation is obtained which closely resembles (1), but which contains an additional term systematically depending on the logarithm of mass. Consequently the residuals $(\log P)_o - (\log P)_c$ derived from (1) were considered, and evidence was obtained that these differences systematically depend on the masses of the stars. The observational data are still too scanty for an accurate determination of the numerical value of the coefficient of log M, but there is sufficient evidence that it must be very near its theoretical value. During the computations it appeared to

* *M.N.*, **91**, 251, 501, 1930–31; *Proc. Roy. Acad. Sci. Amsterdam*, **33**, 9, 1930.
† C. T. Elvey, *Astr. Journ.*, **71**, 4, 1930.

be very desirable to reconsider the scale of our diameters. In order to express them in solar diameters a small correction had to be applied. Then a new relation, which gives the period of axial rotation as a function of the diameter and the mass of a star, was obtained. It appeared that this new relation is valid for the stars in Elvey's list, for the spectroscopic binaries and even for the sun and the different planets. From the spread of the individual differences O − C it was evident that a considerable gain in accuracy is obtained in this way. The details concerning these and other points are given below.

§ 2. *The Theoretical Period of Axial Rotation.*—The theory of the rotation of a shrinking star has been very comprehensively treated by Jeans.* During the contraction the angular momentum of a star must remain a constant, and so if the shrinkage of the star is uniform the angular velocity of the different layers changes uniformly and we always have

$$\frac{d}{dt}(k^2\phi) = 0 \quad . \quad . \quad . \quad . \quad (2)$$

where k is the distance to the axis of rotation. Therefore ϕ, the angular velocity, will always be proportional to k^{-2}. Consequently we must expect that during contraction the relation between the period of axial rotation of a star and its diameter is of the form

$$\log P = 2 \log D + \text{const.} \quad . \quad . \quad . \quad (3)$$

On the other hand, we know from Poincaré's theorem † that whatever the arrangement of the mass, a rotational speed greater than that given by the equation

$$\phi^2/2\pi\gamma\bar{\rho} \leqslant 1 \quad . \quad . \quad . \quad . \quad (4)$$

is inconsistent with steady rotation. With the different star models considered by Jeans the function $\phi^2/2\pi\gamma\bar{\rho}$ is of fundamental importance. Thus in discussing incompressible masses Jeans finds the Maclaurin spheroid to become unstable when the rotation is given by

$$\phi^2/2\pi\gamma\bar{\rho} = 0{\cdot}19.$$

If ϕ^2 increases further a breaking-up process may set in. A rotating mass after Roche's model will pass through a series of configurations, rotating as a rigid body, until ϕ^2 reaches a critical value, viz. $\phi^2 = 0{\cdot}37 \times 2\pi\gamma\bar{\rho}$. As ϕ increases further, the central mass shrinks, but as it shrinks it leaves behind it successive rings of particles revolving in its equatorial plane. At any instant the angular velocity of the central lenticular mass is given by $\phi^2 = 0{\cdot}37 \times 2\pi\gamma\bar{\rho}$. Summarising, we have that at first the period of axial rotation of a shrinking star rapidly decreases, so that the period is proportional to R^{-2} (equation 3) until $P = \frac{k}{\phi}$ reaches a certain critical value. If R decreases further the value of P is then given by the equation (5),

* *Astronomy and Cosmogony*, p. 279 and following.

† *Leçons sur les hypothèses cosmogoniques*, p. 22.

$$P^2 2\pi\gamma\bar{\rho} = \text{const.} \quad . \quad . \quad . \quad . \quad (5)$$

So (5) indicates the period of axial rotation for all stars for which a sufficient interval of time has elapsed since they have originated and for which the initial rotation was different from zero. Presumably this will be the case with the large majority of the observed stars. The numerical value of the constant in this equation is open to some doubt and depends on the star model which is adopted. Later on we shall see that neither of the theoretical values agrees with the one derived from observation. As soon as the development of the star has progressed so far that the limit (5) is reached, either equatorial shedding of matter sets in or a binary system is formed. Immediately after fission the periods of axial rotation and of orbital revolution will be identical. So we expect the relation (5) also to be valid for the recent binary systems. Finally we write instead of (5),

$$2 \log P = -\log \bar{\rho} + \text{const.},$$

or, since $\bar{\rho} = M/kD^3$, we have

$$\log P = 1{\cdot}5 \log D - 0{\cdot}5 \log M + \text{const.} \quad . \quad . \quad (6)$$

where M indicates the mass of the star. Comparing (1) and (6) we see that the relations are almost identical, but the theoretical formula contains an additional term systematically depending on mass.

§ 3. *The Influence of the Term, log M, on the Observed Values.*— I have in the first place tried to ascertain whether an influence of the term log M can be traced in the distribution of the observed values of log P. From (1) the values $\Delta = \log P - 1{\cdot}5 \log D$ are computed. (From now on the value 1·50 instead of 1·49 will be used as coefficient of log D.) If our reasoning was correct the values Δ must systematically depend on log M. Therefore we put

$$\Delta = A \log M + B.$$

The values log M are easily obtained from Eddington's well-known mass-luminosity relation. Next, for the different groups of stars the coefficients A and B are found by least squares solutions. The results are as follows :—

$\Delta = -\ {\cdot}20 \log M - {\cdot}61$ for Elvey's stars.
$\Delta = -\ {\cdot}17 \log M - {\cdot}98$,, eclipsing variables.
$\Delta = -\ 1{\cdot}10 \log M - {\cdot}11$,, spectroscopic binaries ($e \leqslant 0{\cdot}05$).
$\Delta = -\ 1{\cdot}28 \log M + {\cdot}20$,, ,, ,, ($0{\cdot}05 < e \leqslant 0{\cdot}15$).

The influence of log M is very evident, but the results as to the numerical values of A and B are rather discordant. If all stars are combined into one set we find $A = -\ 0{\cdot}67$, *i.e.* nearly the theoretical value, but evidently not much weight can be attached to a mean value obtained in this way. Moreover, the observed range of log M is rather small, and therefore I think that for the present at least we had best adopt the theoretical value 0·50 for the coefficient of log M. Then it remains

only to determine the numerical value of B. For the different stars we find

$\log P = G - \cdot26 \pm \cdot42$	*from Elvey's stars ($G = 1{\cdot}5 \log D - 0{\cdot}5 \log M$).		
$\log P = G - \cdot28 \pm \cdot26$	,,	eclipsing variables.	
$\log P = G - \cdot04 \pm \cdot22$	,,	spectroscopic binaries	($e \leqslant 0{\cdot}05$).
$\log P = G + \cdot29 \pm \cdot40$	,,	,,	($0{\cdot}5 < e \leqslant 0{\cdot}15$).
$\log P = G + \cdot72 \pm \cdot61$	,,	,,	($0{\cdot}15 < e \leqslant 0{\cdot}30$).
$\log P = G + 1{\cdot}24 \pm \cdot76$	,,	,,	($\cdot30 < e \leqslant 0{\cdot}60$).
$\log P = G + 1{\cdot}58 \pm \cdot73$	,,	,,	($e > 0{\cdot}60$).

We remark the increase of the numerical value of the constant term $k = f(e)$ with the older spectroscopic binaries, *i.e.* those with large eccentricities. If from the values mentioned before the function $k = f(e)$ is drawn, the value $k = f(o)$ may be read from the resulting graph. It is found to be $-\cdot18$. At the moment of fission, which is the moment when the parent star has attained its limiting rotation, the eccentricity of the orbit of the resulting binary system must be small or even zero. So from the values derived for the different groups of spectroscopic doubles we conclude that the limiting value of the rotation is given by

$$\log P = 1{\cdot}5 \log D - \cdot5 \log M - \cdot18.$$

From the stars in Elvey's list and from the eclipsing variables the numerical values of the constant term were found to be $-\cdot26$ and $-\cdot28$ respectively. As a final result the mean of these three values was adopted, and so we have

$$\log P = 1{\cdot}5 \log D - 0{\cdot}5 \log M - 0{\cdot}25 \quad . \quad . \quad . \quad (7)$$

§ 4. *Correction to be Applied to the Diameters.*—The way in which we have obtained our diameters was explained in a previous † paper. Briefly summarised, we proceeded in the following way. From Hertzsprung's formula ‡

$$5 \log \sin \rho = \text{const.} + 2{\cdot}5(c_2/T)^{0{\cdot}93} - m \quad . \quad . \quad (8)$$

we obtained hypothetical semidiameters. They were reduced to absolute values by the use of spectroscopic and other parallaxes. These absolute values were expressed in solar diameters by multiplying them by a certain constant. This constant was derived from stars for which the diameters were known from interferometer measures. Obviously the determination is based on a very small number of stars. Moreover, the diameters derived from the interferometer measures themselves had to be reduced to absolute values by using spectroscopic parallaxes. According to van Rhijn § the spectroscopic parallaxes of the bright stars are affected by a systematic error, these parallaxes being too large. Therefore it is very probable that our diameters require a

* $\pm\cdot42$ is the P.E. of the individual deviations.
† *M.N.*, **91**, 501, 1931.
‡ *Annalen Sterrenwacht Leiden*, Deel xiv., eerste stuk, 1922.
§ *Gron. Publ.*, 34 (1923) and 37 (1925).

constant correction. It should be emphasised that this correction should only be applied to the absolute values of the diameters and not to the relative ones derived from (8).

TABLE I.

Binary Systems with known Dimensions.

Star.	Observed.		Computed.		R_1. O−C.	R_2. O−C.
	log R_1.	log R_2.	log R_1.	log R_2.		
V Pup.	0·91	0·87	0·95	0·95	−0·04	−0·08
γ Cyg.	0·67	0·67	0·87	0·83	−0·20	−0·16
U CBr.	0·41	0·72	0·37	−0·23	+0·04	+0·95
Z Vul.	0·58	0·68	0·45	−0·03	+0·13	+0·71
u Her.	0·64	0·72	0·61	0·13	+0·03	+0·59
σ Aql.	0·55	0·55	0·65	0·53	−0·10	+0·02
TV Cas.	0·41	0·38	−0·27	−0·69	+0·68	+1·07
U Oph.	0·46	0·55	0·53	0·47	−0·07	+0·08
RS Vul.	0·63	0·75	0·45	−0·35	+0·18	+1·10
β Aur.	0·42	0·42	0·11	0·11	+0·31	+0·31
WW Aur.	0·28	0·28	0·05	−0·05	+0·23	+0·33
RX Her.	0·15	0·15	−0·83	−0·65	+0·98	+0·80
TX Her.	0·20	0·20	0·05	−0·03	+0·15	+0·23
S Ant.	0·23	0·11	−0·45	−0·93	+0·68	+1·04
Z Her.	0·48	0·35	0·13	0·01	+0·35	+0·34
RS CVn.	0·78	0·88	0·03	0·01	+0·75	+0·87
W UMa.	−0·10	−0·10	−0·25	−0·51	+0·15	+0·41

Only for a limited number of stars are fairly reliable data as to their absolute dimensions available. These are given in Table I. This table contains the different binary systems for which both the spectroscopic and the photometric elements are determined. The columns 2 and 3 give the logarithms of the observed semi-diameters. The computed semi-diameters were derived in the usual way. The colours and the values c_2/T corresponding to the different spectral types and absolute magnitudes, which we want for our calculations, were taken from our diagram in *M.N.*, **91**, 247, 1930. Usually only the spectral type of the brighter component is given, and it was therefore assumed that both components have the same spectral type. The logarithms of the computed semi-diameters appear in columns 4 and 5. The next two columns give the differences O − C. The mean of the differences O − C is + ·25 for the brighter and + ·51 for the fainter components. Evidently our assumption that both components have equal spectral type is not correct. The diameters of the fainter components have been underestimated, *i.e.* their colour indices have been taken too small. From Hertzsprung's formula (8) we easily find that the systematic difference of 0·26 would be completely accounted for if the colour indices of the fainter components were taken − ·35 larger than those of the brighter

ones. This would in the mean correspond to a difference in spectral type slightly less than one spectral class. It is very dubious whether such a difference could easily be detected with these stars. For the present, however, it is useless to go into any details concerning this. Only the spectral types of the brighter components being given, the correction derived from these must be used. Applying the correction + ·25 for log D to formula (7) we finally obtain

$$\log P = 1{\cdot}5 \log D - 0{\cdot}5 \log M - 0{\cdot}62 \quad . \quad . \quad (9)$$

I am aware of the fact that several uncertainties still may affect the absolute values of my diameters. For instance, it is possible that an error systematically depending on spectral type has been introduced. As stated in my previous paper,* the mean curves giving the relations between mean colour, spectral type, and absolute magnitude are badly determined for the very early types. Now any uncertainties affecting these mean colours will also influence the computed values of the diameters (see formula (8)). With the binaries in Table I., 9 B-stars occur. For these the mean difference O − C is + ·07. For the four A-type binaries and the four of types F and G, the corresponding differences are + ·42 and + ·48. Owing to the scantiness of the material we are not able to state definitely whether the run of these numbers is real or accidental, so the general mean value + ·25 was used when deriving (9). With a view to the results to be derived presently it is of importance to remark, that if actually the run of the mean values O − C systematically depends on spectral type, this implies that relatively to the diameters of the later types those of the earlier types are overestimated.

§ 5. *Statistical Results.*—The new relation (9) was applied to the different groups of stars, viz. the spectroscopic binaries with different values of the eccentricity e of the orbit, to the eclipsing variables, to the β Canis-Majoris stars, and to the Cepheids. Owing to the fact that no reliable data as to the absolute magnitudes, colours, and masses of the long-period variables are available, these stars were omitted from the present discussion. For the same reason the RV Tauri stars have not been considered.† The stars used in the different groups are the same as were used in my previous paper. The diameters used there have been corrected in the way described before. The masses of the different stars were derived from Eddington's mass-luminosity relation. Then the values $(\log P)_o + \frac{1}{2} \log M$ were computed. These latter values were plotted against the logarithms of the diameters. The results appear in fig. 1. In this figure the relation (9) is indicated by a full drawn line. From the figure we see that now the division of the P/D diagram into two distinct parts is almost complete. The upper right side of the diagram contains all stars which have been recognised as genuine double stars. The lower left part contains the Cepheids and the β Canis-Majoris stars. The regions containing the doubles

* *M.N.*, **91**, 501, 1931.
† *Ibid.*; and *Proc. Roy. Acad. Sci. Amsterdam*, **33**, 9, 1930.

and the variables are closely adjacent but are never overlapping. For the interval of log D here considered the division line between the two parts seems to be a straight line.

There can remain little doubt as to the close connection between the Cepheids and the β Canis-Majoris stars. The areas covered by these two groups of variables gradually merge into one another, and the points indicating the two groups are distributed in a similar way round one and the same straight line. Therefore in the next sections the Cepheids and the β Canis-Majoris stars are considered as members of one and the same group of stars.

Thus two classes of stars are to be considered: (*a*) the spectroscopic binaries and the eclipsing variables, (*b*) the intrinsic variables.

The stars mentioned under (*a*) are considered in section 6, while the others are treated in section 7.

§ 6. *The Spectroscopic Binary Systems.*—In fig. 1 different symbols are used for indicating the spectroscopic binaries with different values of the eccentricity. We see that the double stars with small eccentricities are scattered within a relatively narrow zone round the line indicating the equation (9). With increasing values of the eccentricity the positive excess of the observed period of revolution over the one computed from (9) is also increasing.

This is just what was to be expected from theory. According to Jeans,* binary stars as a class have started with low values of the eccentricity *e*, which are raised by interaction with other stars. On the other hand, the period of orbital revolution increases as the mass of the star dissolves into radiation. According to Jeans † the increase of the period is proportional to $(M + M_1)^{-2}$. Therefore, if for different masses M the mean differences O − C are computed, the following must be expected: (*a*) the mean values O − C will increase with increasing values of the eccentricity *e*; (*b*) this increase will be largest with the stars of small masses.

For the stars of different masses the results appear in fig. 2. This figure needs no further explanation. We see that the distribution of the mean values O − C is such as would be anticipated if a secular decrease were adopted. The effect of the secular decrease of mass has especially been investigated by H. Vogt ‡ and G. Shajn.§ According to my personal opinion ‖ it must always be difficult definitely to establish such an effect, as our observational data are always influenced by various selection effects. Still, Shajn thinks that notwithstanding the possible influence of selection the effect of the secular decrease of mass may be traced in the various distribution curves of the visual binaries.

Anyhow, the results here obtained are most easily explained as an effect of such decrease. Also I think that in the present case the effect of selection must be small. The chance of discovery of a spectroscopic

* *Astronomy and Cosmogony*, p. 298.
† *Loc. cit.*, p. 293. ‡ *Zeitschr. für Phys.*, **26**, 135.
§ *M.N.*, **85**, 245, 1925; and *A.N.*, **237**, 61, 1929.
‖ *A.N.*, **238**, 374, 1930.

binary must increase if the semi-amplitude K of the velocity curve increases. This semi-amplitude is proportional to

$$(1 - e^2)^{\frac{1}{2}} P^{-1} M \left(\frac{\gamma}{\gamma + 1}\right)^3 \quad . \quad . \quad . \quad (10)$$

where γ represents the mass ratio. Considering different groups of binaries with equal mean values of the eccentricity but with different masses, we see from (10) that with decreasing masses the chance of discovery decreases unless the numerical value of $\gamma/\gamma + 1$ increases.

TABLE II.

Distribution of the Values $(log\ P)_o - (log\ P)_c$ *for Spectroscopic Binaries of Different Spectral Types.*

Δ. / Spectrum.	−1·5.	−1·0.	−0·5.	−0·25.	0·00.	+0·25.	+0·50.	+1·00.	+1·50.
O–B2	0	5	1	1	7	1	4	0	1
B3–B9	0	2	2	9	9	5	7	4	4
A	0	0	1	4	7	8	12	4	7
F	0	0	3	4	3	7	15	6	10
G	0	0	0	1	5	3	7	5	7
K and M	0	0	0	0	2	4	8	2	3

TABLE III.

Mean Values of $(log\ P)_o - (log\ P)_c$, $\bar{e}$ *and* $\bar{\gamma}$ *for Different Masses* (cf. *fig.* 2).

log. Mass. / e.	+1·0. $\overline{O-C}$.	$\bar{e}$.	$\bar{\gamma}$.	0·50. $\overline{O-C}$.	$\bar{e}$.	$\bar{\gamma}$.	0·25. $\overline{O-C}$.	$\bar{e}$.	$\bar{\gamma}$.	$\overline{O-C}$.	$\bar{e}$.	$\bar{\gamma}$.	$\bar{\gamma}$ all Masses.
0·00	−·05	·027	·77	−0·10	·031	·69	0·00	·023	·84	−0·04	·025	·78	·77
0·05	−·05	·092	·54	−0·01	·093	·76	−0·03	·108	·69	+0·47	·090	·87	·75
0·15	+·53	·227	·64	+0·90	·214	..	+0·76	·267	·98	+0·53	·211	·96	·89
0·30	−·22	·403	..	+1·03	·433	·87	+1·25	·473	·66	+1·77	·431	·76	·73
0·60 +	+·52	·740	..	+0·66	·723	·73	+2·17	·788	·57	+2·22	·697	·71	·66
$\bar{\gamma}$ for Σe			·68			·73			·74			·80	

Considering the values in Table III., where I give for stars with different masses the mean values $(\log P)_o - (\log P)_c$ and the mean values $\bar{e}$ and $\bar{\gamma}$, it appears that the mean $\bar{\gamma}$ is almost a constant with the different groups. It should, however, be remarked that γ is only given for a limited number of stars. Still I think the evidence is sufficient to allow the statement that if a selection effect were present, we should expect a decrease of log P with decreasing M and not an increase as is observed. Consequently the results obtained in fig. 2 and in Table III. must be considered as real. In fig. 2 abscissæ are mean values log e (e = eccentricity), ordinates the mean values $\overline{O-C}$. The values for stars

with log mass $> 1{\cdot}0$ are represented by black dots connected by lines. Circles with dots indicate stars with $0{\cdot}50 < \log m < 1{\cdot}00$. Open circles indicate pairs with $0{\cdot}25 < e < 0{\cdot}50$. Double circles are valid for $\log m < 0{\cdot}25$.

We next consider the general distribution of the values O − C. For the separate spectral types the results appear in Table II. As would be anticipated, positive deviations are more frequent than negative ones. Only with the very early spectra there still occur several large negative excesses. Yet the introduction of the mass-term in formula (9) has largely improved our results, as appears when comparing Table II. with the one previously derived without taking into account the influence of mass. In the first place, the numerical amount of the negative excesses is smaller and the differences − 1·00 have altogether disappeared. In the second place, the frequency of the negative deviations has decreased.

The remaining still rather large frequency of the negative deviations with the O and B stars may be caused by a systematic error of the mean colours (see section 4). A few additional words must be said concerning the well-known spectroscopic system ϵ Aurigæ. According to H. Ludendorff * a secondary oscillation with a period of about 150 days occurs in the velocity curve of this star. Using Ludendorff's value we find for $(\log P)_o - (\log P)_c$ a value + ·71, which is quite within the limits of probable error.

§ 7. *The Cepheids and the β Canis-Majoris Stars.*—We consider in fig. 1 the distribution of the different dots and circles which represent these stars. We see that they are scattered within a narrow zone round a straight line which apparently is parallel to the line representing the equation (9). So it seems that there is a close correlation between the observed periods of the variables and the computed ones. This is not quite unexpected, as the theoretical formula which has been derived for the periods of the Cepheids is very similar to (9). The cause of Cepheid variation may be either pulsation or fission, but both possibilities lead to the result that the period is a function of density, so analytically we have $P^2\rho = k$, where ρ again is the mean density of the star. Instead of this we write

$$\log P = 1{\cdot}5 \log D - 0{\cdot}5 \log M + \text{const.} \quad . \quad . \quad (11)$$

The equations (9) and (11) are identical, but for the numerical value of the constant term. We may also say that both the limiting period of axial rotation and that of pulsation (in the fission theory half the period of rotation) are functions of the mean density. Hence the correlation between the different groups of stars is readily understood. Fig. 1, however, gives no single clue which might enable us to decide whether the pulsation or the fission theory is the correct one, nor does it prove that the observed correlation between the double stars and the variables is a direct physical one. To avoid misunderstanding I want to state that I do not think that this correlation is an accidental one only. The fact that both distributions are represented

* *Sitz. d. Kgl. Preuss. Ak. d. Wiss.*, 1902, p. 1068.

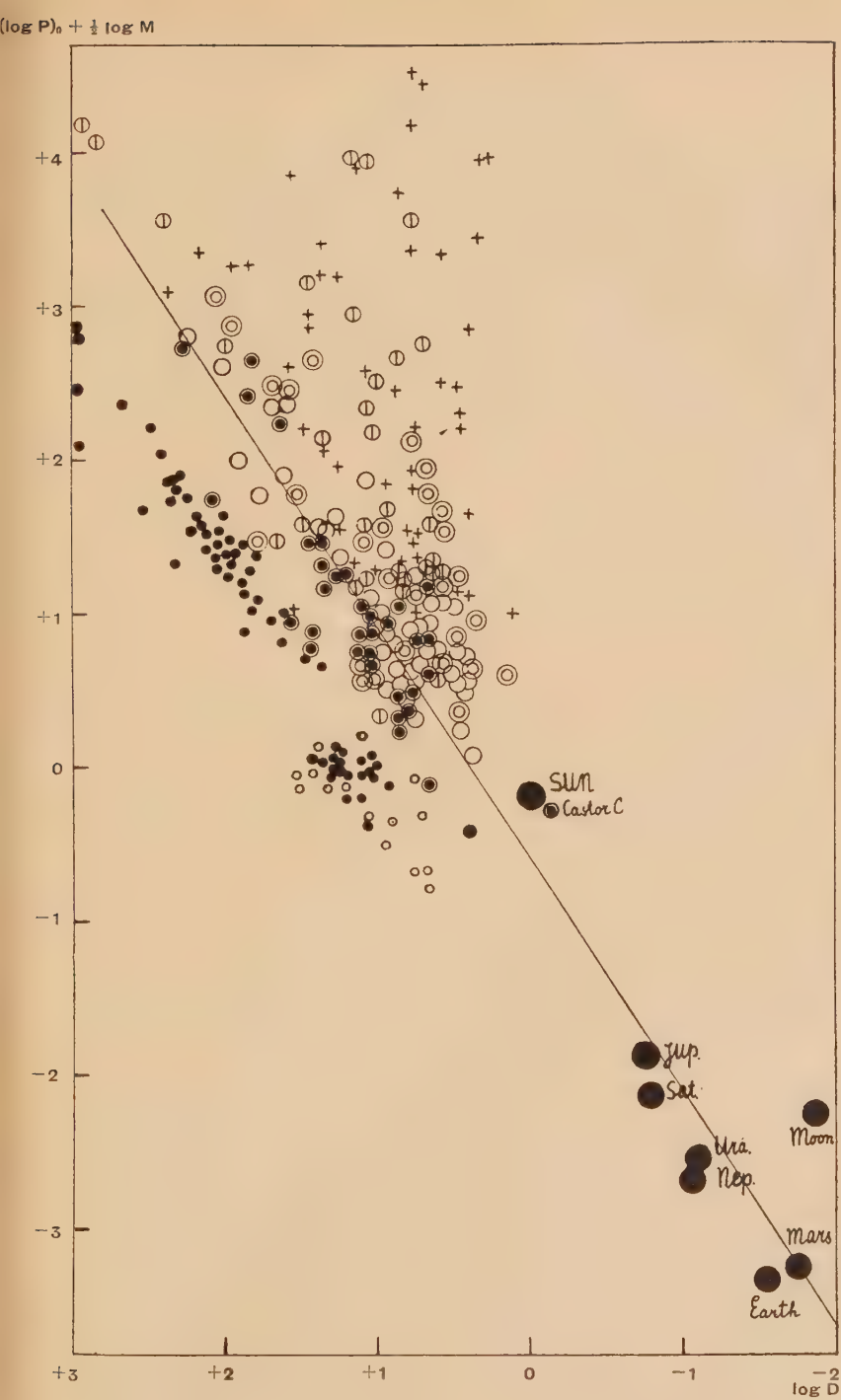

FIG. 1.—The value of $(\log P)_0 + \frac{1}{2} \log M$ plotted against $\log D$.

● = Cepheid variables; ○ = β CMa stars; ⊙ = eclipsing variables; ◯ = spectroscopic binaries with $e < 0{\cdot}05$; ◎ = spectroscopic binaries with $0{\cdot}05 < e \leqslant 0{\cdot}15$; ◍ = spectroscopic binaries $0{\cdot}15 < e \leqslant 0{\cdot}30$; + = all spectroscopic binaries with $e < 0{\cdot}30$.

by almost identical formulæ indicates that some connection exists. But I doubt whether from fig. 1 alone we might conclude that the Cepheids represent an intermediate stage in the development of a binary system, although a close connection between the two groups of stars is also indicated by Struve's results.*

§ 8. *The Relation between the Variables and the Spectroscopic Binaries.* —On the fission theory the period of light variation would be half that of rotation. Therefore immediately after fission the logarithm of the period of orbital revolution of a binary star must be + ·30 larger than

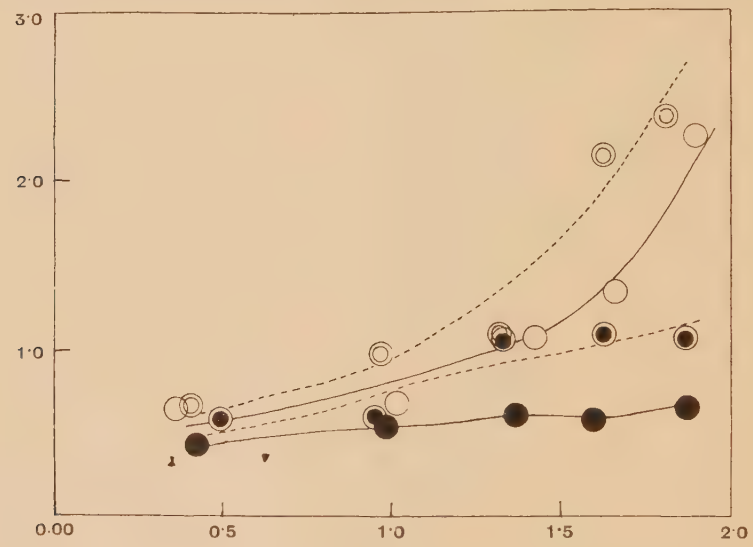

Fig. 2.—The influence of secular decrease of mass on the mean value $(\log P)_0 - (\log P)_c$.

Abscissæ, mean values log e (e = eccentricity); ordinates, mean values O − C.
●——● = stars with log mass > 1·0; ⊙ = stars with 0·50 < log m < 1·00;
○ = pairs with 0·25 < e < 0·50; ◎ = log m < 0·25.

the log of period of light variation of the variable from which the system has originated. From fig. 1 we see that if an upward shift of + ·30 is given to the points representing the variables, a slight additional shift to the right (*i.e.* a small contraction) would be sufficient to cause a complete overlapping of the regions covered by the variables and the newly formed binaries. So far there is no objection to assuming the binary systems to have developed out of the variables. But evidently we must now expect that the masses of the binary systems along the curve representing the equation (9) are equal to those of the variables for which log P is ·30 smaller, while their spectral types are earlier.†

* *Astroph. Journ.*, **60**, 167, 1924; and *M.N.*, **86**, 63, 1925.

† *Cf.* Jeans, *M.N.*, **85**, 797, 1925; and Ten Bruggencate, *Naturw.*, **14**, 905, 1926.

With small modifications this reasoning must also be correct if the double stars originate out of pulsating stars.

As long as only mean masses and " mean spectral types " are considered a fairly good agreement between theory and observation is obtained. The spread of the individual values of the mass are, however, very large. With the longer periods, doubles occur with masses considerably smaller than those of the Cepheids of slightly shorter period, and, conversely, large masses occur with the short-period systems. Therefore the relation between period and mass must be inspected more closely. For this purpose fig. 3 was drawn. In this figure the values $(\log P)_o$ were plotted against the corresponding values of log M. Different symbols were used to indicate the Cepheids, the β Canis-Majoris stars, and the spectroscopic binaries of the various spectral types. Now a star of a certain mass increases its angular velocity when shrinking. So in fig. 3 the star must progress along a vertical line to the bottom (if for the present a possible increase or decrease of mass is neglected). When the period of axial rotation attains its limiting value the following may occur: (*a*) an equatorial shedding of matter sets in; (*b*) the star breaks up into a binary system; (*c*) the star develops into a Cepheid variable.

Now let us suppose that the Cepheid stage is an intermediate stage in the breaking-up process. When breaking up, the mass of the system must remain constant. Therefore the period of orbital revolution of the resulting system must be larger (by 0·30) than the period of the Cepheids of equal mass, and this positive excess must increase as the mass of the double stars dissolves into radiation. So for a given mass the period of the Cepheids should indicate the minimum period which is compatible with this mass. Consider again fig. 3. For the various spectral types the minimum periods as observed for different masses are connected by faintly drawn or dotted curves. Obviously the periods of orbital revolution of the early type spectroscopic binaries are shorter than the period of light variation of the Cepheids of equal mass. This is especially the case with the larger masses. On the other hand, the orbital periods of the later type doubles are longer than those of the Cepheids. Consequently if the Cepheid stage is an intermediate stage in the breaking up process of a star, this intermediate stage is at least not a compulsory one.

Until now only the axial rotation of a shrinking star has been considered, but we have no single proof that Cepheid variation directly or indirectly is an effect of rotation. On the contrary, from the theoretical work of Eddington we know that as by contraction a star proceeds along the giant sequence towards the earlier spectral types, it may at a certain moment, quite independently of any rotation, develop into a Cepheid. Summing up, we have that a contracting star may develop either into a binary system or into a Cepheid. (There are of course other alternatives, *e.g.* equatorial shedding of matter.) The question whether a binary or a variable is the result may depend on the initial properties. If, for instance, the initial angular velocity is large, Poincaré's limit may be reached before the star has been able

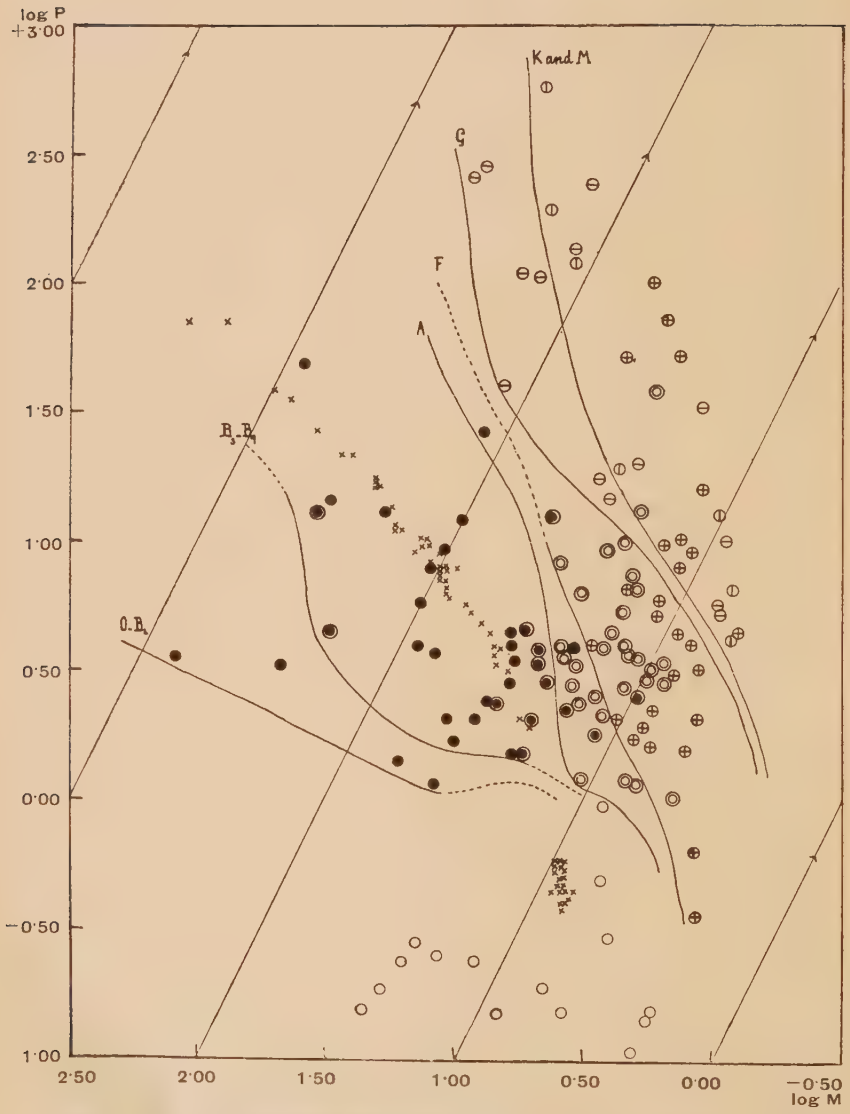

FIG. 3.—The period of revolution and of light variation as a function of Mass. Of the binary systems only those with $e \leqslant 0{\cdot}15$ are included in the diagram.

× = Cepheids; ◯ = β CMa stars; ⊙ = binaries with spectral type Bo–B7; ◎ = A-type binaries; ⊕ = F-type binaries; ⊖ = G-type binaries; ◑ = K and M-type binaries.

to develop a Cepheid character. On the other hand, if the initial velocity is small, the internal condition favourable for Cepheid variation may occur before the period attains its limiting value, and the result will be a variable. In the latter case the star ultimately may still break up into a double, when its angular velocity has sufficiently increased. For the present these latter points should be considered as a working hypothesis only.

§ 9. *The Periods of Axial Rotation of the Sun and the Planets.*—In section 2 we saw that as a star contracts its angular velocity increases up to a certain limit. For the stars the numerical value of the limit is computed from (9).

The values of mass and diameter are quite arbitrary, and so we expect equation (9) to be valid for any body which is freely rotating in space. With the sun and the different planets the couple which arises from the gravitational pull of tides on tides falls off approximately as $(1/r)^6$, where r is the distance of a planet from the sun. Therefore the influence of tidal friction in the solar system must be negligible, and the sun and the planets may be considered as bodies freely rotating in space. Consequently, with some restrictions, equation (9) must also represent the rotational periods of the various bodies in our system. Some restrictions should be made, as the surface conditions of the stars and the planets are widely different. In Table IV. the observed periods are

Table IV.

The Observed Periods of Sun, Earth, and Planets compared with those derived from (9). *For the Observed Periods of the Sun and the Earth, see Text.*

	$(\log P)_0$.	$(\log P)_c$.	O−C.		$(\log P)_0$.	$(\log P)_c$.	O−C.
Sun	−0·36!	−0·61	+0·25	Jupiter	−0·39	−0·59	+0·20
Mercury	..	−0·91	..	Saturn	−0·36	−0·43	+0·07
Venus	..	−0·89	..	Uranus	−0·35	−0·61	+0·26
Earth	−0·57	−0·91	+0·34	Neptune	−0·49	−0·59	+0·10
Mars	0·01	−0·93	+0·94	Moon	1·44	−0·84	+2·28

compared with those derived from (9). Concerning the "observed" periods of the earth and the sun the following should be remarked. Fouché * estimated the total angular momentum of the solar system to be 28·2 times that of the sun, the latter being supposed to be a homogeneous mass rotating uniformly in the period of the outer layers. According to Jeans † the total angular momentum may be as much as 60 times larger. The effect of concentrating all this angular momentum in the sun would be to cause it to rotate 60 times as fast as now, so its period of axial rotation has been taken equal to 26/60 days. In the same way the angular velocity of the earth would increase if the total angular momentum of the earth-moon system were concentrated in the earth. The observed period used in Table IV. is the one which is computed in that way. With the other planets no corrections have

* *Comptes Rendus*, **99**, 903, 1884.

† *Astronomy and Cosmogony*, p. 15. See also T. J. J. See, *A.N.*, 4053, 1905.

been applied to the observed periods as the masses of the various satellites are but imperfectly known. Taking into account the fact that the values of the coefficients in formula (9) were derived for stars with masses several thousand times larger than those of the planets, we see that the agreement between the observed and the computed values is rather good. As a rule small positive excesses of the observed periods over the computed ones occur. This might be due to the fact that there remains a small systematic error in the determination of the absolute values of our diameters (see section 4). For the moon the excess of the observed value over the computed one is very large, but here tidal friction has increased the period. With the planets the largest positive excess occurs with Mars. I do not think, however, that too much weight should be attached to the individual values. The surface conditions as well probably as the internal conditions of the different planets are widely different, and therefore no very accurate results can be expected. The point of chief interest, however, is that the periods computed from (9) are very near the mark. To what extent this is the case is best shown by fig. 1, where the sun and the planets have been indicated by large dots. We conclude that equation (9) must be valid for a very large range of diameters and masses.

With the inner planets our knowledge as to the periods of axial rotation is very limited. For Mercury a period of a few hours is indicated by the thermo-electric measures of E. Pettit and S. B. Nicholson.* For Venus periods of $1^d\ 10^h\ 34^m$ and 68^h † have been derived by A. Belopolski ‡ and W. H. Pickering. From the results in Table IV. we would also conclude that the rotational periods cannot be very much in excess of a few hours.

§ 10. *Multiple Systems.*—In section (2) we have seen that as a star contracts its angular momentum must remain constant and its angular velocity increases proportionately to D^{-2}. When its period of axial rotation has attained a certain limit, *i.e.* the one given by (9),

$$\log P = 1{\cdot}5 \log D - 0{\cdot}5 \log M - 0{\cdot}62 \quad . \quad . \quad (9)$$

the star may break up into a double system. Let the masses of the components of the resulting binary system be M_1 and M_2, and their diameters D_1 and D_2. Immediately after fission (if fission really occurs) the periods of axial rotation of both components must be equal to the period of orbital revolution, while the latter must be equal to the period of rotation of the parent star. On the other hand, the periods of axial rotation of the individual components must be larger than, or at least equal to, the limiting values which for those components are derived from Poincaré's equation. In the latter case one or both components may immediately split up into a binary and the result is a multiple system. In the former case the distance between the two original components must increase until the influence of tidal friction is negligible. Then contraction may again cause an acceleration of the angular velocity

* *Pop. Astron.*, **33**, 299, 1925.
† *Publ. Obs. Cent. Nicolas*, **11**, 1911.
‡ *B.A.A.J.*, **31**, 218, 1921.

until the limit (9) is attained for one of the components. When this occurs this component may break up. From the foregoing it is evident that the following conditional equations must be satisfied:—

$$\left.\begin{aligned} \log P - \log P_1 &= 1{\cdot}5 \log \frac{D}{D_1} - 0{\cdot}5 \log \frac{M}{M_1} \geqslant 0 \\ \log P - \log P_2 &= 1{\cdot}5 \log \frac{D}{D_2} - 0{\cdot}5 \log \frac{M}{M_2} \geqslant 0 \end{aligned}\right\} \quad . \quad (12)$$

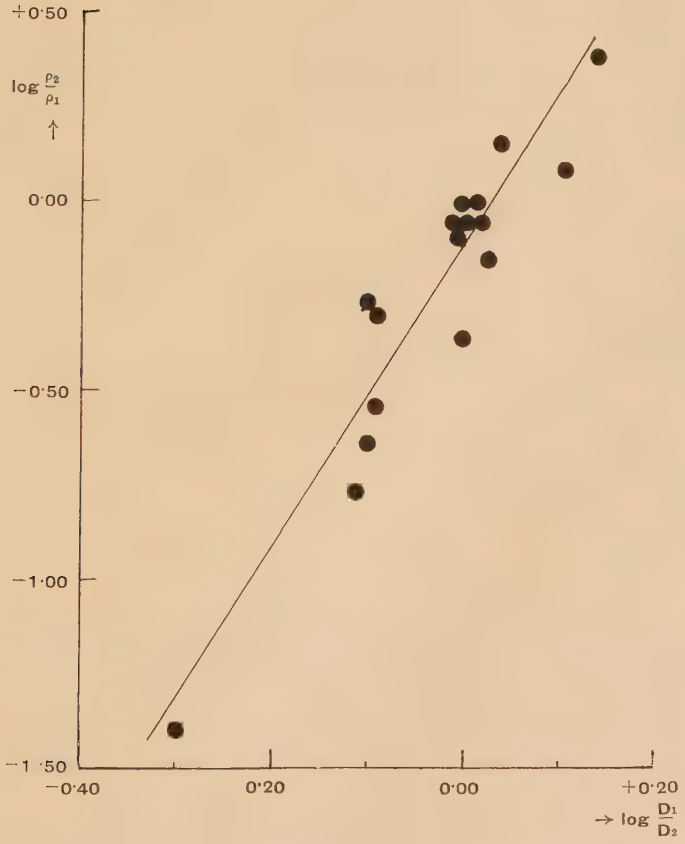

FIG. 4.—The ratio of the mean densities as function of the ratio of diameters.

In order to obtain from (12) some further information as to the way in which double and multiple systems may originate, some additional observational data must be considered. We know that as a rule the brighter component of a spectroscopic double has the smaller diameter and the larger density. Reliable data concerning the masses and diameters are available only for the few stars enumerated in Table I.

If for those stars the logarithms of the ratios of densities are plotted against the corresponding logarithms of the diameters, an approximately linear relation is obtained (see fig. 4). If we put

$$\log \rho_2/\rho_1 = A \log D_1/D_2 + B \quad . \quad . \quad . \quad (13a)$$

we find from a least squares solution,

$$\log \rho_2/\rho_1 = 4{\cdot}00 \log D_1/D_2 - {\cdot}12 \pm {\cdot}10 \text{ (error of single deviation)} \quad (13b)$$

Of course formula 13 is to be considered as an interpolation formula only. Its intrinsic value cannot be very large, as the number of well-determined stars is limited and we have no absolute proof that a linear relation actually exists. Moreover, it must be difficult to find a wholly unobjectionable way of discriminating between the components 1 and 2. Instead of (13*b*) we write

$$\log M_2/M_1 = \log D_1/D_2 - {\cdot}12 \quad \text{or} \quad M_2/M_1 = \kappa D_1/D_2 \quad . \quad (14)$$

We put

$$\left.\begin{aligned} D_1 &= \alpha D \\ D_2 &= (1-\alpha)D \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (15)$$

then we have

$$M_1 = \frac{(1-\alpha)}{1-(1-\kappa)\alpha}M\,; \qquad M_2 = \frac{\kappa\alpha}{1-(1-\kappa)\alpha}M \quad . \quad (16)$$

and from (12), (15), and (16) we have

$$\left.\begin{aligned} \phi(\alpha) &= -3\log\alpha + \log\frac{(1-\alpha)}{1-(1-\kappa)\alpha} \geqslant 0 \\ \psi(\alpha) &= -3\log(1-\alpha) + \log\frac{\kappa\alpha}{1-(1-\kappa)\alpha} \geqslant 0 \end{aligned}\right\} \quad . \quad (17)$$

Hence if a star breaks up into a double, the diameter α of the first component (unit = the diameter of the parent star) must be such that the conditions (17) are simultaneously fulfilled. From fig. 5, where the functions $\phi(\alpha)$ and $\psi(\alpha)$ are graphically represented, it appears that this is the case in the interval $0{\cdot}39 \leqslant \alpha \leqslant 0{\cdot}71$. Each separate value α in this interval, and its complementary value $(1-\alpha)$ indicates a possible course of evolution. Now the following is evident :—

(*a*) From (17) and (12) we see that the value $\phi(\alpha)$ corresponding to a given value of α indicates the excess of the period of axial rotation (P_1 = P) of the first component over its limiting value (9). In the same way $\psi(\alpha)$ indicates the corresponding excess of the second component.

(*b*) The first component cannot break up again unless the difference $1{\cdot}5 \log D_1 - 0{\cdot}5 \log M_1$ corresponding to it has decreased by $\phi(\alpha)$. With the second component a decrease of $1{\cdot}5 \log D_2 - 0{\cdot}5 \log M_2$ equal to $\psi(\alpha)$ is necessary.

(*c*) If the masses of the different components are adopted to be

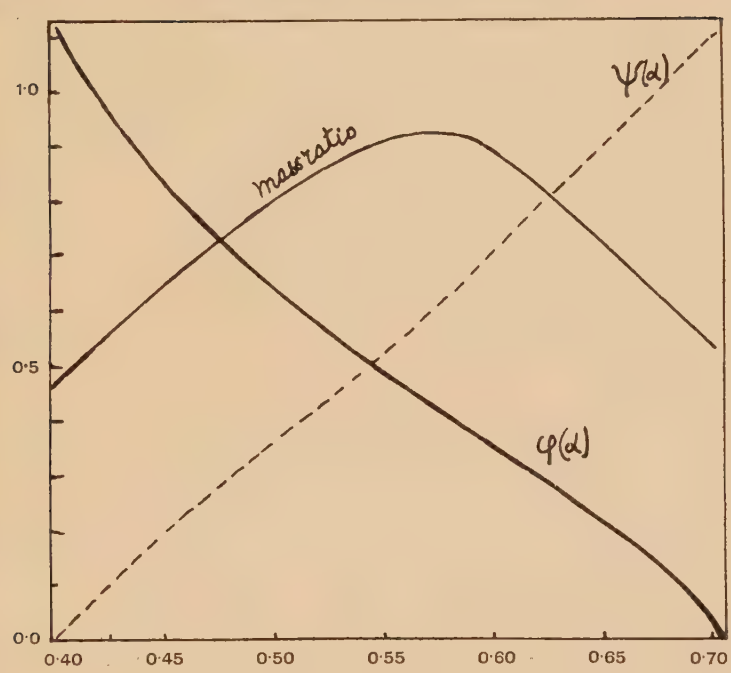

FIG. 5.—The functions $\varphi(\alpha)$ and $\psi(\alpha)$ and the mass ratio as functions of α and $(1 - \alpha)$ respectively.

constant, then each component must contract while increasing its angular velocity until

$$\Delta(1{\cdot}5 \log D_1) = -\phi(\alpha) \quad \text{and} \quad \Delta(1{\cdot}5 \log D_2) = -\psi(\alpha).$$

(*d*) This decrease of $\Delta(\log D)$ implies an increase of the mean density. Therefore the component cannot break up again before its density has increased in a certain ratio. Some numerical data are given in Table V.

TABLE V.

The Functions $\varphi(\alpha)$ and $\psi(\alpha)$ and the Theoretical Masses, Diameters, and Mass Ratios in Binary Systems.

α.	$\phi(\alpha)$.	$\psi(\alpha)$.	M_1.	M_2.	M_1/M_2.	$\bar{\rho}_1$.	$\bar{\rho}_2$.	Ratio of Contraction, c_1.	Ratio of Contraction, c_2.
0·39	1·12	0·00	0·68	0·32	0·47	11·4	1·4	5·6	1·—
0·45	0·83	0·20	0·61	0·39	0·64	6·6	2·3	3·6	1·4
0·50	0·66	0·36	0·57	0·43	0·75	4·6	3·4	2·7	1·7
0·55	0·48	0·52	0·52	0·48	0·92	3·1	5·2	2·1	2·2
0·60	0·34	0·71	0·47	0·53	0·89	2·2	0·3	1·7	3·0
0·65	0·20	0·90	0·42	0·58	0·72	1·5	13·3	1·3	4·0
0·71	0·00	1·10	0·35	0·65	0·54	1·0	26·6	1·—	5·4

The first three columns of this table give the values of α and the corresponding values of $\phi(\alpha)$ and $\psi(\alpha)$. Columns 4 and 5 give the masses of the separate components (unit = the mass of the parent star) and column 6 the mass ratio. The mean relative densities appear in columns 7 and 8. Finally, column 9 indicates the ratio of contraction necessary for a new break up. Evidently the possibility of a new break up must be largest when for one of the components either $\phi(\alpha)$ or $\psi(\alpha)$ is small. (If one of these functions is small for a certain value of α the other will be near its maximum value.) So if the primary components have a large mass ratio and a new breaking-up process occurs the result must be a triple system. With the intermediate values of α, $\phi(\alpha)$ and $\psi(\alpha)$ are not widely different and the chance of a second break up is almost equal for the two components. From the foregoing reasoning and from the numerical values in Table V. the following general conclusions may be drawn.

1. The small mass ratios of the components of double systems occur with the extreme values of α and $(1 - \alpha)$. The more extreme the values α and $(1 - \alpha)$ are, the more unequal the components will be.

2. Immediately after fission the masses of the components are nearly inversely proportional to the diameters (equation 14).

3. The ratio of contraction necessary for producing a second fission is larger for the greater mass.

4. The ratio of the dimensions at first and second fission is always small.

5. As a rule the increase of density between first and second fission is large.

6. If A, B_1, and B_2 indicate the components of a triple system, the mass ratio $(B_1 + B_2)/A$ must be small. With quadruple systems the mass ratio $(B_1 + B_2)/(A_1 + A_2)$ will be near unity.

These various conclusions should be considered as qualitative ones only. For quantitative results the relation between the relative densities (masses) and diameters must be far better known. We remark that the mass ratios which occur in the solar system are far outside the limits in Table V., so it is not probable that the different planets and satellites have originated by breaking-up processes. The results mentioned in points 1–6 compare fairly well with those previously derived by H. N. Russell.* Point 3, however, is in direct contradiction with his results. Finally it should be remarked that if the Cepheid stage is an intermediate one in the process by which a spectroscopic double originates, we must expect spectroscopic doubles to occur which at the same time are Cepheids. I do not know that hitherto such systems have been detected.

§ 11. *The Period of Axial Rotation as a Function of Spectral Type and Absolute Magnitude.*—Table VI. gives, for the various spectral types and the different absolute magnitudes, the limiting periods of axial rotation computed from the equation (9) (the masses are computed from Eddington's mass-luminosity relation). Consider a star which, while contracting, proceeds along the giant series towards the earlier

* *Astroph. Journal,* **31**, 185, 1910.

Table VI.

The Limiting Period of Axial Rotation as a Function of Absolute Magnitude and Spectral Type.

$M_{abs.}$ / Spectrum.	−6.	−4.	−2.	0.	+2.	+4.	+6.	+8.	+10.	+12.
M	..	..	2·71	2·16	1·62	1·27	0·62	0·14	−0·34	0·79
K	..	..	2·67	2·10	1·14	0·60	−0·07	−0·36	−0·84	
G	..	..	2·30	1·53	1·02	0·41	−0·11	−0·58	−1·05	
F	..	..	1·59	1·07	0·57	0·08	−0·41	−0·89		
A	..	..	1·10	0·46	0·18	−0·31	−0·80			
B	..	1·17	0·74	0·38	−0·04	−0·53	−1·02			
O	1·45	1·10	0·73	0·31						

spectral types. Evidently a breaking-up process may set in as soon as its period of axial rotation has decreased so far that it is equal to its limiting value (9) (and so the rate of increase of the angular velocity must be larger than the rate of decrease of the limiting period. From the equations in section 2 we easily see that this really occurs). If the initial velocity (angular) is so large that the breaking-up process sets in when the spectral type is still M, the limiting value of P is a function of absolute magnitude only. With a smaller initial velocity a breakage can occur only when the star has progressed farther towards the earlier types. But whether the breakage occurs when the spectral type is still M or F . . . B or O, within each spectral type the limiting period remains a function of absolute magnitude only. We again take into account the fact that immediately after fission the period of axial rotation of the parent star must be identical with that of orbital revolution. We then conclude that with the spectroscopic doubles of the different spectral types, the minimum period of orbital revolution must be a function of absolute magnitude, so that the largest minimum values occur with the brighter systems. Over the whole range of magnitudes the curve giving the minimum periods of the late-type stars must indicate higher values than the curves corresponding to the earlier types. To what extent these conclusions are correct appears from fig. 6, where the observed periods are plotted against the absolute magnitudes. Different symbols have been used for indicating the various spectral types. The curves representing the different minimum functions are indicated by full-drawn and dotted lines. The general run of these curves is such as was anticipated.

Considering again the numbers of Table VI., we see that along the main sequence the change of the minimum period is rather small, so we conclude that the absolute minima of log P which occur with the various spectral types are not widely different. This also is confirmed by our figure.

Next consider a star which proceeds along the main sequence towards the later types. Its diameter changes but little, but a considerable decrease of mass occurs. Hence the chance that a binary system will

originate is small. Summing up, we have that the binary systems will chiefly originate along the giant series. Moreover, considering the percentages of the total numbers of giants of the different spectral types, these percentages must increase with the earlier types. This agrees with the statistical results obtained in a previous paper.* Along

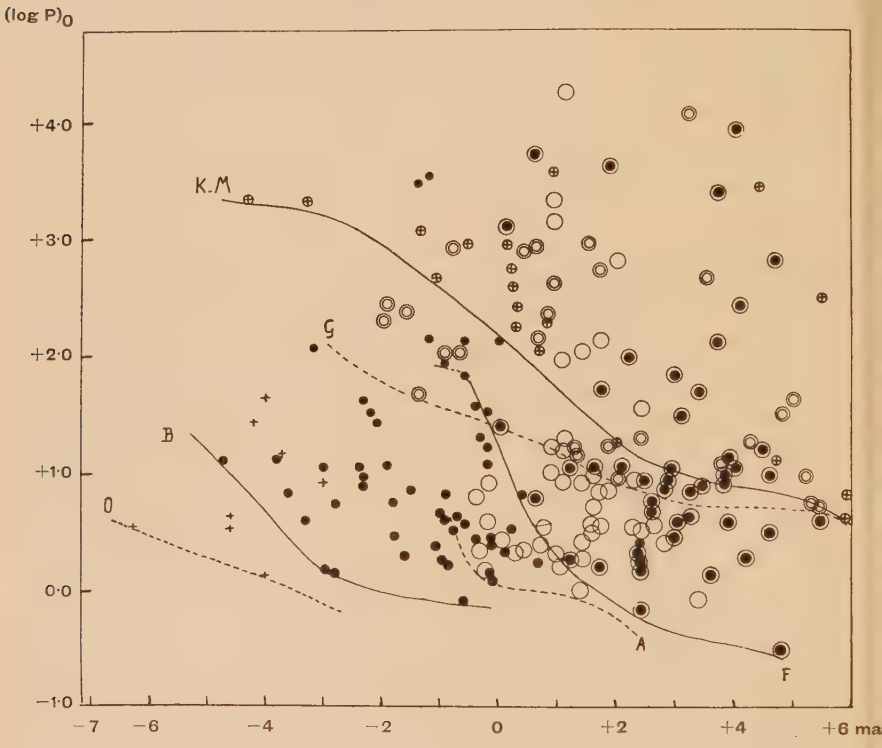

FIG. 6.—The period of revolution of spectroscopic binaries (and eclipsing variables) as a function of absolute magnitude and spectral type.

\+ = O stars; ● = B stars; ○ = A stars; ⊙ = F stars; ◎ = G stars; ⊕ = K and M stars.

the main sequence the percentage of stars which are spectroscopic doubles must practically remain constant.

§ 12. *Some Final Remarks.*—In many respects the results derived in the present paper are indications only. Still I think that the fundamental importance of the relation (9) has been proved beyond any doubt. There of course remain many difficulties. We remark, in the first place, the difficulty of explaining the large spread of the individual values of log P round the mean curve. There may be an additional factor which influences the period of rotation, *e.g.* the degree of central condensation.

* *M.N.*, **90**, 212, 1929.

Next there is the systematic run of the mean differences with spectral type, which may be caused either by a systematic error of the colours used by us or by the degree of central condensation in the different spectral types. In the third place, we remark that the numerical value of the constant term in (9) is much smaller than its theoretical value.* There can be no doubt that a clearer insight as to the intrinsic value of (9) will be gained as soon as more observational material is available. Especially important will be the determination of the rotational velocity for an additional number of stars. Owing to the large spread of the angles, only mean values of the rotational velocity can be used. Therefore in the present paper Elvey's list was of small use.

Evidently we must expect to obtain similar results to those of the present paper when the periods of axial rotation are derived from direct measurements instead of being taken equal to the periods of orbital revolution. A checking of the results must, however, be very desirable, but for that we want more extensive material. I am pleased to mention that Dr. G. Shajn has kindly informed me that perhaps a part of the observational work at the Simeis Observatory may be devoted to the study of the contours of the spectral lines. Thus in due time we may expect additional data to become available. Finally, it is evident that for the problems arising from stellar rotation the observation of the short-period spectroscopic systems is of almost equal importance.

Bandoeng :
1931 *April.*

The Spectrum of Nova Pictoris, 1926–1931.

By H. Spencer Jones, F.R.S., H.M. Astronomer at the Cape.

An account of the spectrum of Nova Pictoris from the day of discovery, 1925 May 25, to 1926 March 13, when observations at the Cape Observatory with the 4-prism spectroscope attached to the 24-inch Victoria telescope were terminated, has been given by Dr. Lunt.† I have prepared a fuller account of the spectrum during this period, based on a more detailed measurement of the plates measured by Lunt, supplemented by the measurement of a large number of additional plates, which will appear shortly in the *Cape Annals*. The discussion of the Cape material has been supplemented by the measurement of a number of objective-prism spectrograms, taken at the Union Observatory, Johannesburg, which were kindly lent to me by Mr. H. E. Wood, Union Astronomer, thus enabling the later spectral

* See Jeans, *Astronomy and Cosmogony*, p. 389.
† *M.N.*, **86**, 498–524, 1926.

changes to be followed. The last plate taken at the Union Observatory was obtained on 1928 October 24. At my request, a further plate was obtained by Mr. Worssell, of the Union Observatory, on 1931 February 21, 22, 23, with a total exposure of $6\frac{1}{2}$ hours. As nothing has yet been published about the later spectrum of Nova Pictoris, a brief summary of the results derived from the study of the objective-prism spectra will be of interest.

The Johannesburg spectra were photographed with the Franklin-Adams telescope (Cooke triplet lens of 6 inches aperture), to which an objective prism with refracting angle of 16° was attached. The plates used were Ilford Special Rapid, Ilford Zenith, Imperial Special Sensitive, Ilford Panchromatic, Agfa Panchromatic, and Cramer Iso-Instantaneous. With the exception of the panchromatic plates, they were bathed in Sensitol Red, Sensitol Green, and Sensitol Violet. The spectra extend, when the exposure is sufficient, from about λ 3700 to λ 6700. The definition is sharp in the two extreme portions of the spectrum but falls off somewhat in the middle range.

The linear scale at various wave-lengths along the spectrum is as follows :—

λ.	Scale. A. per mm.	λ.	Scale. A. per mm.
3700	29·3	5500	125·8
4000	40·2	6000	166·4
4500	63·0	6500	212·3
5000	91·9		

The plates were measured in the Toepfer measuring machine and reduced with the aid of the formula :

$$\lambda = \lambda_0 + \frac{c}{(n_0 - n)^\alpha},$$

in which n denotes the micrometer reading, λ_0 and α are constants for the prism, c depends upon the scale of the plate, and n_0 upon the position of the plate in the measuring machine. The best fit was obtained with $\alpha = 0{\cdot}84$, $\lambda_0 = 1801{\cdot}4$, and these values were adopted. For each plate the constants n_0 and c were chosen to provide the best fit for the lines $H\alpha$, $H\beta$, and $H\delta$. The formula provides a surprisingly good representation of the measures throughout the spectrum, but there are small systematic deviations which were determined approximately by means of lines whose identification is free from doubt. This method of reduction necessarily assumes that the hydrogen lines are unblended and that the constant of the Doppler displacement of the hydrogen lines is the same as the constants for the other lines. These conditions appear to be very closely satisfied.

Three objective-prism spectra were obtained also at the Cape Observatory in 1928 July and August and 1929 February. These were obtained with the $7\frac{1}{2}^\circ$ objective prism in front of the 24-inch objective of the Victoria telescope. The Cape spectra extend from about λ 3750

to λ 5725, but the focus is poor at the two ends of the spectrum. They have been measured and discussed by Mr. H. Horrocks, chief assistant. The results of the measures are included in Table II. with those of the Johannesburg plates for 1928.

It may be recalled that when the Cape observations terminated on 1926 March 13 the spectrum of the nova was almost entirely an emission band spectrum. The hydrogen absorptions were visible but very weak. They had been progressively weakening for some while. The emission bands included strong hydrogen bands, the helium bands, λλ 3889, 3965, 4026, 4144, 4388, 4471, 4713, 4922, and 5016, the "forbidden" emissions of ionised iron which are prominent in the spectrum of η Carinæ, together with normal emissions of ionised iron and chromium. The following nebular bands were also present: λλ 3869, 4069, 4363, 4658, 4959 (N_2) and 5007 (N_1). The widths of the bright bands were very closely proportional to the wave-lengths, being represented by the formula:

$$\text{Width (in A.)} = 27{\cdot}19 \times 10^{-4}.$$

The metallic emissions were becoming weaker in 1926 March, the forbidden emissions at a slower rate than the normal emissions. No trace of ionised helium, λ 4686, nor of the nebular band, λ 4640, could be detected in the spectrum up to March 13. The Johannesburg plates of early 1926 show the following emissions beyond the limit of the Cape plates: *Fe*$^+$, λλ 5169, 5276, 5317, 5338; [*Fe*$^+$], λ 5273;* *He*, λλ 5876, 6678; *Hα*; and the nebular emission frequently observed in the spectrum of novæ at λ 5755.

The Spectrum of Nova Pictoris, 1926 *March-October.*

Johannesburg spectra are available for the following dates in 1926, subsequent to the termination of the Cape observations: March 20, 28; April 4, 12, 28; May 11, 17, 25; June 4, 20; July 21; September 7, 17; October 4. The measures of these spectra are summarised in Table I. Estimated intensities, on a purely arbitrary scale, are given; it should be mentioned, however, that the intensities of some lines decreased during this period, whilst others increased, and that some intensities varied irregularly. Some of the lines were visible for only a portion of the period.

The general constitution of the spectrum may be summarised as follows:—

Hydrogen.—The hydrogen lines from *Hθ* to *Hα* were measured. *Hα*, *Hβ* are very strong. The intensities of the remaining lines show a progressive decrease with wave-length.

Helium.—Neutral helium is well represented in the spectrum. The lines at λλ 4472, 5876 are very strong. λλ 4026, 4144, 4713 are of moderate strength. Other lines undoubtedly present include λλ 3819, 4121, 4388, 4922, and 6678. The λ 3867 line is blended with the strong

* The forbidden emissions are denoted, for convenience, thus: [*Fe*$^+$].

TABLE I.

The Emission Band Spectrum of Nova Pictoris, 1926 March–October.

Measured λ.	Identification.	Tabular λ.	Obs.–Tab.	Intensity.	Multiplet.	Remarks.
3760·2	Neb. [O^{++}]	3759·9	+ ·3	1	$3k^3P_2$–$3m^3D_3$	
3797·9	$H\theta$	3797·9	·0	1	2^2P°–10^2D	
3818·0	*He*	3819·6	−1·6	0	1^3P°–5^3D	
3836·2	$H\eta$	3835·4	+ ·8	2	2^2P°–9^2D	
3840·0	Neb.	3840·2	− ·2	1		
3868·7	*He*	3867·5	+1·2	30	1^3P°–5^3S	Mainly nebular line.
	Neb.	68·7	·0			
3889·0	*He*	3888·7	+ ·3	20	1^3S–2^3P°	
	$H\zeta$	89·0	·0	..	2^2P°–8^2D	
3893·4	..	..	..	3		
3968·7	*He*	3964·7	+4·0	35	1^1S–3^1P	
	Neb.	67·5	+1·2			
	$H\varepsilon$	70·1	−1·4	..	2^2P°–7^2D	
4004·9	..	..	..	0		
4025·9	He^+	4025·6	+ ·3		74^2F–13^2G°	
	He	26·2	− ·3	..	1^3P°–4^3D	
4070·3	Neb.	4068·6	+1·7	6		
4099·0	N^{++}	4097·5	+1·5	6	$3k^2S$–$3m^2P_2$	
4101·7	$H\delta$	4101·7	·0	45	2^2P°–6^2D	
4114·7	..	..	..	1		
4120·9	*He*	4120·8	+ ·1	2	1^3P°–4^3S	
4144·0	*He*	4143·8	+ ·2	5	1^1P°–5^1D	
4178·9	Fe^+	4178·9	·0	3	$b^4P_{2\frac{1}{2}}$–$z^4F^\circ_{3\frac{1}{2}}$	Intensity too great in later stages to be due entirely to Fe^+.
4202·5	He^+	4199·9	+2·6	4	4^2F–11^2G°	
4207·5	..	..	..	0		
4235·1	Fe^+	4233·2	+1·9	1	$b^4P_{2\frac{1}{2}}$–$z^4D^\circ_{3\frac{1}{2}}$	
4243·8	[Fe^+]	4244·0	− ·2	3	$a^4F_{4\frac{1}{2}}$–$a^4G_{5\frac{1}{2}}$	Stronger in later stages. Another emission involved.
4277·0	[Fe^+]	4276·9	+ ·1	1	$a^4F_{3\frac{1}{2}}$–$a^4G_{4\frac{1}{2}}$	
4286·0	[Fe^+]	4287·4	−1·4	3	$a^6D_{4\frac{1}{2}}$–$a^6S_{2\frac{1}{2}}$	
4340·5	$H\gamma$	4340·5	·0	85	2^2P°–5^2D	
4363·1	Neb. [O^{++}]	4363·2	− ·1	30	a^1D–a^1S	
4384·5	*He*	4387·9	−3·4	2	1^1P°–4^1D	
4414·7	[Fe^+]	4413·8	+ ·9	4	$a^6D_{2\frac{1}{2}}$–$a^6S_{2\frac{1}{2}}$	
	[Fe^+]	16·3	−1·6	..	$a^6D_{4\frac{1}{2}}$–$b^4F_{4\frac{1}{2}}$	
4454·8	[Fe^+]	4452·1	+2·7	1	$a^6D_{1\frac{1}{2}}$–$a^6S_{2\frac{1}{2}}$	
	[Fe^+]	58·0	−3·2	..	$a^6D_{3\frac{1}{2}}$–$b^4F_{3\frac{1}{2}}$	

TABLE I.—*continued.*

Measured λ.	Identification.	Tabular λ.	Obs.–Tab.	Intensity.	Multiplet.	Remarks.
4471·6	*He*	4471·5	+ ·1	25	$1^3P^\circ–3^3D$	
4514·8	Fe^+	4515·3	− ·3	1	$b^4F_{2\frac{1}{2}}–z^4F^\circ_{2\frac{1}{2}}$	Broad blend, including also Fe^+ 4508, 20, 22, and possibly N^{++}, 4511, 15.
4543·1	He^+	4541·6	+ 1·5	1	$4^2F–9^2G^\circ$	
4570·1	? Neb.	4571·5	−1·4	1	..	Observed only on Sept. 7.
4580·4	Fe^-	4583·8	−3·4	0	$b^4F_{4\frac{1}{2}}–z^4D^\circ_{3\frac{1}{2}}$	
4606·8	..	..	..	1		
4637·3	N^{++}	4634·3	+3·0	10	$3m^2P_1–3n^2D_2$	
	N^{++}	40·8	3·5	..	$3m^2P_2–3n^2D_3$	
4656·3	Neb.	4658·2	−1·9	3		
4685·7	He^+	4685·8	− ·1	35	$3^2D–4^2F^\circ$	
4713·4	*He*	4713·1	+ ·3	5	$1^3P^\circ–3^3S$	
4723·9	Neb.	4725·5	−1·6	2		
4771·2	..	..	..	0		
4813·0	$[Fe^+]$	4814·5	−1·5	0	$a^4F_{4\frac{1}{2}}–b^4F_{4\frac{1}{2}}$	
4861·3	*Hβ*	4861·3	·0	85	$2^2P^\circ–4^2D$	
4899·9	..	..	..	2		
4917·9	*He*	4921·9	−4·0	2	$1^1P^\circ–3^1D$	
4959·6	Neb. $[O^{++}]$	4958·9	+ ·7	3	$a^3P^\circ_1–a^1D_2$	
5007·9	Neb. $[O^{++}]$	5006·8	+1·1	9	$a^3P^\circ_2–a^1D_2$	
	He	15·7	−7·8	..	$1^1S–2^1P^\circ$	
5041·2	..	..	..	3		
5148·6	..	..	..	2		
5172·7	Fe^+	5169·0	+3·7	3	$a^6S_{2\frac{1}{2}}–z^6P^\circ_{3\frac{1}{2}}$	Stronger on later plates. Probably mainly Fe^+ on earlier plates, but mainly of unknown origin on later plates.
5232·5	..	..	..	1		
5271·4	$[Fe^+]$	5273·4	−2·0	2	$a^4F_{4\frac{1}{2}}–b^4P_{2\frac{1}{2}}$	Probably a blend with line of unknown origin.
5310·8	..	..	..	2		
5412·6	He^+	5411·6	+1·0	2	$4^2F–7^2G^\circ$	
5720·9	..	..	..	6	..	Strong on 1926 Sept. 7; absent from other plates.
5752·7	Neb.	5754·8	−2·1	3		
5875·6	*He*	5875·7	− ·1	15	$1^3P^\circ–2^3D$	
6087·5	..	..	..	2	..	Stronger on later plates.
6562·9	*Hα*	6562·8	+ ·1	80	$2^2P^\circ–3^2D$	
6677·3	*He*	6678·2	− ·9	2	$1^1P^\circ–2^1D$	

nebular line at λ 3869 and λ 3964 overlaps the $H\epsilon$ emission, producing a band conspicuously broader than adjacent bands. λ 5015 overlaps the N_1 band, but the measured wave-length indicates that the nebular emission is predominant in the blend. In 1926 September and October a perceptible diminution in the intensities of the emissions of neutral helium is noticeable.

Ionised Helium.—The most prominent line is the λ 4686 line. As mentioned above, up to 1926 March 13 no trace of this line could be seen in the spectrum. Wright * has drawn attention to the sudden appearance of the λ 4686 line between March 8 and 15. Davidovich † has also reported that no trace could be seen of this line on the Arequipa plate of March 13. On March 15 the λ 4686 line is reported by Wright to be the strongest in the spectrum after $H\beta$ and $H\gamma$. From being absent or so weak on March 13 as not to be apparent on the Cape or Arequipa photographs, the He^+ line, λ 4686, flashed out by March 15 into one of the dominant bands of the spectrum. The strength of this line after its appearance was very variable.‡ It was very strong on March 20 and 22, but by March 28 there was a considerable decrease in the intensity, though the line was still strong. On April 4, 5, 12 the line was weak, but early in May it was strengthening again. By the middle of May it was once more very strong, only to become considerably weaker by May 25. Observations on June 4, June 20, and July 21 show the emission to have been fairly strong on those dates. The next plate obtained was on September 7, when λ 4686 was as intense as $H\beta$. On September 17 it was somewhat weaker than $H\beta$. On October 4 it was weaker than on September 17, but was the strongest emission after $H\alpha$, $H\beta$, and $H\gamma$. Thus throughout the period from its first appearance to the last plate of 1926, this emission was characterised by rapid and considerable fluctuations in intensity. Apart from the λ 4686 line, there are no prominent emissions due to ionised helium, though several members of the Pickering series are present, but of low intensity.

Ionised Iron.—Ionised iron is represented in the spectrum by a number of ordinary bands and also by a number of "forbidden" bands, the latter being generally stronger than the former. Multiplet designations are given in Table I. As pointed out by Merrill,§ the "forbidden" lines all arise from transitions from even terms of intermediate energy-levels to the two lowest terms, a^6D and a^4F, which are also even terms. Such transitions are forbidden by the empirical rules, derived from the analysis of laboratory spectra. Several of these metallic emissions persist or become stronger when the others have faded away and are probably blended with other emissions. The Fe^+, λ 4179, emission is weak in 1926 March and April, but subsequently strengthens. An emission of approximately the same wave-length is present in the spectrum in 1928, when metallic emissions

* *P.A.S.P.*, **38**, 233, 1926. † *H.C.O. Bulletin*, No. 839, 1926.

‡ The summary which follows is based on the Johannesburg plates and the reproductions of Lick spectra given by Wright (*loc. cit.*).

§ *Ap. J.*, **67**, 213, 1928; *Mt. Wilson Contributions*, No. 354.

have almost entirely disappeared. It would seem that another emission, at about λ 4180, appears about 1926 May ; its origin is unknown. The [Fe^+] emission at λ 4243·8 is as strong in 1926 October as in 1926 March, and is still stronger in 1928 and of rather longer wave-length. In the latter year the line is undoubtedly mainly of some other origin. Fe^+, λ 5169·0, strengthens appreciably in 1926 September and October, and the measured wave-length increases ; in 1928 there is a moderately strong line at about λ 5172 which is also present, and of about the same intensity, in 1931 ; this emission is not due to Fe^+. The [Fe^-] emission at λ 5273 also appears to be involved with another emission ; a fairly strong line, of approximately the same wave-length, is present in 1931. The emission measured at λ 4515 is a broad emission which probably involves the Fe^+ lines at λλ 4508, 4515, 4520, 4522 and possibly, in addition, the lines due to doubly ionised nitrogen at λλ 4511, 4515.

Nebular Emissions.—The following nebular emissions are probably present : λλ 3759·9, 3840·2, 3868·7, 3967·5, 4068·6, 4097·5, 4363·1, 4571·5, 4634·1, 4640·8, 4658·2, 4725·5, 4958·9, 5006·8, 5754·8.

The λ 3759·9 emission is attributed by Bowen to doubly ionised oxygen. This emission was first seen on 1926 March 20. It was never prominent during 1925 but is moderately strong in the spectra of 1928 and 1931.

The λ 3840·2 emission appears as a weak wing to the $H\eta$ band. Comparison with the other hydrogen bands indicates that it is not a true wing but a weak overlapping band. The measured wave-length is based on the extent of the overlap and agrees closely with the wave-length, measured by Wright, of a line in the spectrum of the envelope of B.D. + 30° 3639. It had probably strengthened in 1928, as the appearance of a wing was not shown, but the measured wave-length of $H\eta$ on both Cape and Johannesburg plates of that year shows the effect of a blend with a line of longer wave-length than $H\eta$. Its origin is not known.

λ 3868·7 is a typical nebular emission of unknown origin which first appeared early in 1926. It increased rapidly in intensity and by 1926 September it was double the intensity of the adjacent $H\zeta$ line. By 1928 the intensity had decreased to approximate equality with $H\zeta$, and in 1931 the line could no longer be detected.

The presence of the λ 3967·5 emission at this stage is uncertain, on account of the proximity of *He*, λ 3964 and $H\epsilon$, λ 3970. By 1928, when the neutral helium lines had weakened, it was undoubtedly blended with $H\epsilon$.

The emission at λ 4068·6 first appeared about 1925 October, at the same time as the forbidden iron bands. It is present in the spectra of η Carinæ and of "iron" stars, such as B.D. − 12° 1500. The measured wave-length shows a gradual increase, and it appears probable that in the later stages another emission of greater wave-length emerges and blends with it.

The λ 4097·5 emission appears as a distinct wing on the short wave-length side of $H\delta$. It is generally attributed to doubly ionised nitrogen. There is another strong line in the spectrum of doubly ionised nitrogen

at λ 4103·5. No trace can be seen of a wing on the long wave-length side of $H\delta$, but a narrow wing, such as this emission would give, would be difficult of detection with the small scale of the objective-prism plates. The principal other lines in the N^{++} spectrum are at $\lambda\lambda$ 4634, 4641; 4511, 4515; 4379. The line measured at λ 4637·3 is probably a blend of the first two. This emission appears in the spectrum at about the same time as He^{+}, λ 4686, as a strong emission line, which persists in 1926 but is no longer visible in 1928. The pair of lines at $\lambda\lambda$ 4511, 4515, if present, are weak and blended with the Fe^{+} group at about the same wave-length. They were not noted by Lunt * as present, along with the other N^{++} bands, in the spectrum of Nova Aquilæ, No. 3. The λ 4379 line behaves differently from other lines in the N^{++} spectrum and is frequently less intense than λ 4640.†

The λ 4363 emission appears early in 1926. Between 1926 March and October it is strong, but weaker than $H\gamma$. By 1928 it has become appreciably weaker, and in 1931 it is doubtfully present as a very weak line. It is attributed by Bowen to metastable O^{++}.

The emission at λ 4570 is a weak emission measured only on the plate of 1926 September 7 (a particularly good plate). The origin may be identical with that of a weak line at λ 4571·5 measured by Wright in the spectra of certain nebulæ. It was not detected in 1928.

The λ 4658 emission was first detected in 1925 December. On the objective-prism plates it appears as a weak radiation falling between $\lambda\lambda$ 4640, 4686. It has disappeared from the spectra by 1928. This line has been observed in the spectra of a number of nebulæ, but its origin is unknown.

The emission measured at λ 4723·9 was first seen on 1926 March 20. By 1926 September it had increased somewhat in intensity though still weak, but it was appreciably stronger in the 1928 spectrum. A line at λ 4725·5 was measured by Wright in the spectrum of N.G.C. 7027, but the line is not normally present in the spectra of nebulæ. The measured wave-length in Nova Pictoris, both in 1926 and 1928, was shorter by about 2 A., so that the identification must be regarded as doubtful.

The emissions at $\lambda\lambda$ 4959, 5007 are the typical nebular bands, N_2 and N_1. They are not a conspicuous feature in the spectrum of Nova Pictoris either at this stage or in 1928. In the latter part of 1926, and also in 1928, the measured wave-length of the N_2 band is appreciably longer than 4959. The nebular band is apparently blended with a stronger radiation of longer wave-length and of unknown origin. On one or two plates the band is suspected of duplicity, but the focus is not sharp at this wave-length. In 1931 no trace can be seen of N_1, but a line is measured at λ 4968 which is probably the radiation that blends with N_2 in 1926 and 1928. Bowen has shown that the N_1, N_2 bands are due to doubly ionised oxygen.

The emission at λ 5753 agrees closely in wave-length with a strong line present in the spectrum of the envelope of the star B.D. + 30° 3639,

* *M.N.*, **80**, 534, 1920.

† Wright, *M.N.*, **81**, 186, 1921; Fowler, *M.N.*, **81**, 190, 1921.

and occasionally observed, though usually of low intensity, in the spectra of nebulæ.* This line has been observed in the spectra of several previous novæ. It was observed by Campbell in the spectrum of Nova Aurigæ; Campbell remarks that in 1892 it was easily observed, but that by 1894 it had become extremely difficult.† In the spectrum of Nova Persei, in its later stages, this line also appeared in the form of a strong band.‡ Wright observed the same line in the spectrum of Nova Geminorum II.; it was observed on 1912 March 18 on the first plate that was obtained covering the visual region.§ The typical N_1, N_2 nebular lines did not appear until a somewhat later date. The same order of development occurred in Nova Pictoris. No Johannesburg plates are available for the interval 1925 July 14 to 1926 January 9. On 1926 January 10 the λ 5755 line was strong but the N_1, N_2 lines did not begin to appear until about the end of January. The origin of this emission is not known with certainty. It has been suggested by Merrill || that it may be the forbidden line (a^1D_2–a^1S_0) of ionised nitrogen, the theoretical wave-length of which is 5754·8. This identification is supported by the disappearance of the line from the later spectrum of Nova Pictoris which corresponds to a higher degree of ionisation; as remarked above, it disappeared also from the later spectrum of Nova Aurigæ.

Other Emissions.—There are a number of other emissions for which no origins have been assigned and which have not been detected in the spectra of nebulæ, viz.: $\lambda\lambda$ 3893·4, 4004·9, 4114·7, 4207·5, 4606·8, 4771·2, 4899·9, 5041·2, 5148·6, 5232·5, 5310·8, 5720·9, 6087·5.

The λ 3893·4 line overlaps the $H\zeta$ band, appearing as a well-defined wing on its long wave-length side. It was first seen on the plate of 1926 March 20, but may have been present at an earlier date as the earlier plates of 1926 are too weak at $H\zeta$ to show it. It was not detected in the 1928 spectrum.

The emission at λ 4004·9 is weak and was suspected on several plates and measured only on that of October 4. The wave-length is very uncertain.

The emission at λ 4114·7 is a weak but well-defined emission lying between $H\delta$ and *He*, λ 4121, and overlapping the latter. It can be seen on all of the Johannesburg plates from 1926 March 20 to October 4 and has been measured on most of them. The wave-length suggests identification with the λ 4116 line of trebly ionised silicon; but, if this were so, the rather stronger line of the same multiplet at λ 4089 should appear. No trace of this line can be seen.

The emission at λ 4207·5 overlaps He^+, λ 4200. It is rather weaker than the ionised helium line. Only on the sharpest plates can it be recognised as an overlapping band; on the other plates the overlap, of stronger intensity than either band, has been measured and in-

* Wright, *Pub. Lick Obs.*, **13**, 242 (Table II.), 1918.
† Campbell, *Ap. J.*, **1**, 49–51, 1895.
‡ Campbell and Wright, *L.O.B.*, **1**, 56, 1901.
§ Wright, *Pub. Lick Obs.*, **14**, pt. ii., 85, 1926.
|| Merrill, *Ap. J.*, **69**, 361, 1929.

terpreted as the He^+ emission. In consequence, the mean wave-length derived for the latter is large by 2·6 A.

λ 4606·8 is a weak and ill-defined emission which in 1928 was still present in the spectrum of the nova, with somewhat increased intensity.

The λ 4771·2 emission was measured on a few plates only. It is weak and ill-defined and the wave-length is somewhat uncertain. It was not detected in the 1928 spectrum.

The emission at λ 4899·9 is in the region of the plate where the focus is poor. This emission is practically continuous with the *He*, λ 4922, band. The wave-length is rather uncertain. In 1928, when the band due to neutral helium had considerably weakened, a moderately strong band was measured at λ 4894 which may be identical in origin with the one measured in the 1926 spectrum at λ 4900. In the 1931 spectrum the same emission was apparently present, the measured wave-length being λ 4899.

The emission at λ 5041·2 is identical with one which was measured at λ 5043·2 in 1926 January and February. It is of moderate strength and is present on all the 1926 plates but does not show on those of 1928 or 1931.

λ 5148·6 is an emission which is not seen on the early plates in 1926 but becomes stronger on the later plates. It is present and of moderate strength in 1928 and, apparently, also in 1931. The wave-length in 1931 is rather discordant, but the emission is blended with another of longer wave-length, is of weak intensity, and difficult to measure.

The λ 5232·5 emission is very weak but measurable on several plates. It was not seen in 1928 or 1931. It may possibly be due to Fe^+, λ 5234·6, the agreement in wave-length being sufficiently close for a weak line in this part of the spectrum. If this identification is correct, the stronger lines of the (a^4G–z^4F°) multiplet at $\lambda\lambda$ 5276, 5317 should also be present. Lines agreeing approximately in position were measured at $\lambda\lambda$ 5271·4, 5310·8. The former is probably due mainly to the forbidden Fe^+ emission, λ 5273·4, though Fe^+, λ 5276, may also be involved. The presence of a weak line in the 1928 spectrum at λ 5274 possibly indicates that a line of unknown origin also contributes. The emission at λ 5310·8 is to the violet side of a line measured in January 1926, and attributed to a blend between the (a^4G–z^4F°) line of Fe^+ at λ 5316·6 and the (a^4G–z^4D°) line of Fe^+ at λ 5316·8. The measures in 1928 give a line at λ 5308, of unknown origin but possibly the same as the line at λ 5305·0 which was measured by Wright in the spectrum of B.D. + 30° 3639. It appears probable that the λ 5310 emission, measured in the later part of 1926, is a blend of the Fe^+ lines with this line of unknown origin.

The emission at λ 5720·9 was seen in 1926 only on the plate of September 7, when it was sharp and of moderate strength. No trace of it can be seen on any other plate in 1926. On September 7 it was rather less intense than the λ 5753 nebular emission, which is typical of novæ. It became a prominent line at a later date and is strong in the spectra of 1928 and 1931. It does not appear to have been measured previously.

The λ 6087 emission was measured on a few plates in 1926 as a very weak emission. It is of great interest as it subsequently increased very greatly in intensity, and in 1931 it had become the strongest line in the spectrum.

The Spectrum of Nova Pictoris in 1928.

The following plates are available for the study of the spectra at this time: Two objective-prism spectra obtained in Johannesburg in 1928 April and October; three objective-prism spectra obtained at the Cape in 1928 July and August and 1929 February. The measures of these plates are summarised in Table II. The measures, reductions, and intensity estimates of the Cape and Johannesburg plates have been carried out entirely independently, the former by Mr. Horrocks, the latter by myself. The wave-lengths show excellent agreement in general. The intensity estimates are not so concordant; it must be remembered, however, that the two scales are arbitrary and independent, and that the Johannesburg spectra are somewhat out of focus in the middle range, whereas the Cape spectra are out of focus in the two end portions.

The spectrum in 1928 is characterised by a great reduction in the intensity of the lines due to neutral helium as compared with 1926, and a marked strengthening of the λ 4686 line of ionised helium. The *He* lines, $\lambda\lambda$ 4472, 5876, both strong in 1926, are weak in 1928; several other lines due to neutral helium which were measured in 1926 cannot be detected in 1928. On the other hand, He^+, λ 4686, has become much the strongest line in the spectrum, and the members of the Pickering series are also stronger than in 1926. The metallic emissions, including the forbidden lines, are much weaker than in 1926, if not entirely absent: the suggested identifications are not free from uncertainty. The spectrum indicates an appreciably higher degree of ionisation than in 1926.

Perhaps the most striking and interesting feature of the 1928 spectrum is the λ 6087 line. As previously mentioned, this line was very weak in 1926. On the Johannesburg plate of 1928 April, intensity 15 was assigned to it, but on the 1928 October plate it was much stronger than *Hα* and was exceeded in strength only by He^+, λ 4686. In 1931 it was the strongest line in the whole spectrum, being then of about double the intensity of He^+, λ 4686. The line has not previously been observed and its origin is unknown. Low density and a high degree of ionisation are apparently necessary for its production.

Of the lines which are attributed by Bowen to doubly ionised oxygen, λ 3760 is considerably stronger than in 1926, whilst λ 4363 is appreciably weaker. The typical N_1, N_2 emissions are of low intensity. The wave-length derived for N_1 is in good agreement with the accepted value, but the wave-length of N_2, as measured on both the Cape and Johannesburg plates, is large. There is undoubtedly another emission present, of longer wave-length than N_2. This is confirmed by the presence of a line at λ 4968 in the 1931 spectrum when no trace can be

TABLE II.

The Emission Band Spectrum of Nova Pictoris, 1928.

Johannesburg Plates.		Cape Plates.		Identification.	Tabular λ.	Obs.—Tab.	
λ.	Int.	λ.	Int.			Johannesburg.	Cape.
3759·8	8	3759·9	20	Neb. [O^{++}]	3759·9	− ·1	·0
3838·7	2	3838·6	4	$H\eta$	3835·4	+3·3	+3·2
				Neb.	40·2	−1·5	−1·6
3869·1	10	3868·4	18	Neb.	3868·7	+ ·4	− ·3
3889·6	10	3890·9	18	$H\zeta$	3889·1	+ ·5	+1·8
3968·5	18	3969·0	25	Neb.	3967·5	+1·0	+1·5
				$H\varepsilon$	70·1	−1·6	−1·1
3989·7	2	3996·3	3				
4005·1	3						
4026·1	2	4025·6	3	He^{+}	4025·6	+ ·5	·0
4072·3	5	4071·6	9	Neb.	4068·2	+3·7	+3·0
				? O^{+}	76·2	−3·9	−4·6
4101·7	25	4101·6	35	$H\delta$	4101·7	·0	− ·1
4143·4	2	4143·9	1	He	4143·8	− ·4	+ ·1
4181·0	3	4180·2	2				
4201·5	5	4199·3	10	He^{+}	4199·9	+1·6	− ·6
4244·8	6	4246·6	8				
4286·9	2	4286·2	1	[Fe^{+}]	4287·4	− ·5	−1·2
4340·7	45	4340·4	50	$H\gamma$	4340·5	+ ·2	− ·1
4363·7	14	4363·7	18	Neb. [O^{++}]	4363·2	+ ·5	+ ·5
4385·5	1	..	..	He	4387·9	−2·4	
4416·3	2	4418·3	1	? O^{+}	4416·	0·	+2·
4471·1	4	4471·6	3	He	4471·5	− ·4	+ ·1
4513·8	1	4516·1	1				
4542·1	7	4541·8	12	He^{+}	4541·6	− ·5	+ ·2
4607·3	3	4605·3	8				
4686·1	100	4686·6	100	He^{+}	4685·8	+ ·3	+ ·8
4722·5	10	4723·3	18	Neb.	4725·5	−3·0	−2·2
4816·7	2						
4861·1	40	4861·1	50	$H\beta$	4861·3	− ·2	− ·2
4894·4	5	4893·9	12				
4939·1	9	4941·8	20				
4963·1	5	4969·8	10	Neb. [O^{++}]+ unknown	4958·9		
5007·5	7	5007·0	6	Neb. [O^{++}]	5006·8	+ ·7	+ ·2
5149·2	5	5147·3	6				
5172·8	7	5171·4	6				
5272·3	2	5274·4	1	[Fe^{+}]	5273·4	−1·1	+1·0
5308·6	5	5302·9	1				
5410·1	2	..	..	He^{+}	5411·6	−1·5	
5723·3	35	5724·6	12				
5874·6	4	..	..	He	5875·7	−1·1	
6087·4	15, 80						
6563·2	50	..	..	Ha	6562·8	+ ·4	

seen of N_1. The presence of such a blend in 1928 is also suggested by the strength of the emission at or near N_2, as compared with that of N_1. The normal ratio for $N_2 : N_1$ is 1 : 2. Some evidence of this blending was also obtained in the latter part of 1926.

Of the other nebular lines, λ 3840 is apparently blended with $H\eta$; the measures of the Cape and Johannesburg plates both show a closer agreement with the wave-length of the nebular line than with that of $H\eta$. The nebular line λ 3868·4 is weaker than in 1926, the reduction of intensity being due only in small measure to the weakening of the *He* line, λ 3867·5, with which it is blended. The effect of the blending of the nebular line at λ 3967·5 with $H\epsilon$ becomes more apparent with the decreased intensity of the *He* line at λ 3964. The measured wave-lengths suggest approximate equality in the intensities of $H\epsilon$ and the nebular line.

The measured wave-length for the nebular line at λ 4068 shows a discordance, on both the Cape and Johannesburg plates, of the order of 3 A. A discordance in the same direction, but of smaller amount, was noted on the later plates of 1926. It is evident that the emission at λ 4072 is a blend of the λ 4068 nebular line with another line at about λ 4076. It is of interest to note that Wright's measures of nebular wave-lengths include a line at λ 4076·2 and that this line and λ 4068 are normally associated with one another, both being present or both absent in the nebular spectrum.* The intensity of λ 4068, in nebulæ, is greater than that of λ 4076. The λ 4076 line is tentatively assigned by Bowen to the spectrum of ionised oxygen.† The only other line in the region covered by the observations which Bowen tentatively assigns to O^+ is a weak line at λ 4416, measured by Wright in N.G.C. 6572. This line belongs to a different multiplet from the λ 4076 line. A weak line at approximately this wave-length was measured on both Cape and Johannesburg plates. In the earlier stages there was an emission due to the forbidden iron lines at $\lambda\lambda$ 4413·8, 4416·3, which would be expected to be very weak in 1928. It is probable that the line at λ 4416 in the 1928 spectrum is mainly of different origin; possibly it is due to O^+. It may be noted that there is a strong line at λ 4416·2 in the spectrum of the nucleus of B.D. + 30° 3639.

The remaining lines in the spectrum are of unknown origin and are not included in Wright's table of nebular wave-lengths. There are several coincidences with wave-lengths of lines in the spectrum of B.D. + 30° 3639, which may be accidental. Lines were measured by Wright at $\lambda\lambda$ 4247, 4516·6, 4940, 5272·6, and 5305·0 which agree closely with the wave-lengths of lines in Nova Pictoris. Other possible coincidences between wave-lengths in the nova spectrum and other spectra are a line at λ 4608 in B.D. + 35° 4013, and a line at λ 4725·5 in N.G.C. 7027.

The weak lines at $\lambda\lambda$ 3989·7, 4005·1 are probably identical with those measured in 1926 at $\lambda\lambda$ 3893·4, 4004·9. The line at λ 4181 is too strong to be attributed to the Fe^+, λ 4178·9, line which was present in the

* Wright, *Pub. Lick Obs.*, **13**, 242 (Table II.), 1918.
† Bowen, *Ap. J.*, **67**, 1, 1928.

spectrum in 1926. Similarly, the λ 4245 line is much too strong to be attributed, as in 1926, to [Fe^+], λ 4244·0. The weak line, λ 4287, is possibly due to the forbidden emission of ionised iron; the λ 4287 line is the strongest of these forbidden lines. It may be noted, however, that a line was measured in the 1931 spectrum, at λ 4288, which can scarcely be attributed to [Fe^+], λ 4287.

The line at about λ 4606 was present but very weak in 1926. The emission at λ 4894 is apparently identical with one measured in 1926 at λ 4900; the latter wave-length was uncertain on account of blending with *He*, λ 4922. The intensity is greater in 1928 than in 1926.

The strong emission at λ 4940 is shown on all the plates from 1928 April to 1929 February. It was not present in 1926. Its origin is unknown, but, as mentioned above, a line of this wave-length is present in the spectra of Wolf-Rayet stars. The line at λ 5172 is identical with the one measured at λ 5175 in 1931; it cannot be attributed to Fe^+, λ 5169. The λ 5273 line is possibly due, in part, to Fe^+, λ 5273, but since the line appears strengthened in the 1931 spectrum it is probably of some other origin. A line of this wave-length is common in Wolf-Rayet spectra.

The emission at λ 5724 is considerably out of focus on the Cape plates, but is sharp and well-defined on the Johannesburg plates. It is very strong in both 1928 April and October. This is the same line as the one measured at λ 5721 on the plate of 1926 September 7, but absent from the other plates of that year. In 1928 it is not greatly inferior to *Hα* in intensity. This line has not been observed in the spectra of nebulæ or Wolf-Rayet stars.

The Spectrum of Nova Pictoris in 1931.

Only one plate is available for the study of the 1931 spectrum, obtained, as mentioned above, at the Union Observatory, Johannesburg, on the nights of 1931 February 21, 22, and 23. The plate is somewhat fogged but the stronger lines are well shown. The weaker lines are diffuse and their measurement is difficult, the measured wave-lengths being correspondingly uncertain. The derived wave-lengths are given in Table III.

Table III.

The Emission Band Spectrum of Nova Pictoris, 1931.

Measured λ.	Int.	Identification.	Tabular λ.	Obs.—Tab.	Remarks.
3758·9	8	Neb. [O^{++}]	3759·9	−1·0	Vague and ill-defined.
4101·7	5	*Hδ*	4101·7	0·0	
4121·2	2	..	..	..	Suspected. ? *He*, 4121·8.
4288·2	2	..	..	..	,,
4340·7	12	*Hγ*	4340·5	+0·2	
4363·2	1	Neb. [O^{++}]	4363·7	−0·5	,,
4620·3	1	..	..	..	Doubtful.

TABLE III.—*continued.*

Measured λ.	Int.	Identification.	Tabular λ.	Obs.—Tab.	Remarks.
4687·0	40	He^+	4686·6	+0·4	
4714·3	2	..	..	..	Suspected. ? *He*, 4713·1.
4861·2	15	$H\beta$	4861·3	−0·1	
4899·0	2	..	..	..	Doubtful.
4942·7	8	..	..	..	Ill-defined.
4968·1	4	..	..	..	,,
4988·8	3				
5152·1	6	..	..	..	Broad, ill-defined blend.
5175·1	5	..	..	..	
5276·5	8				
5310·8	6				
5413·3	1	He^+	5411·6	+1·7	Measure uncertain.
5722·6	30				
6088·4	75				
6562·7	40	$H\alpha$	6562·8	−0·1	

The only identifications which can be made with safety are those of the hydrogen lines, the λ 4686 line of ionised helium, and the λ 3760 nebular line, attributed to doubly ionised oxygen. The λ 4363 nebular line, also attributed to doubly ionised oxygen, is apparently present but is extremely weak.

Comparing this plate with the plate of 1928 April 25, with which it is comparable in that the $H\alpha$ lines are of about the same intensity, the following points of resemblance or difference between the two spectra are noticed :—

(1) The strength of the continuous spectrum is about the same on the two plates.

(2) The decrease in intensity of the lines of the Balmer series towards the violet is very much greater in 1931 than in 1928. The intensities of $H\beta$, $H\gamma$, $H\delta$ are respectively in 1931 about $\frac{1}{2}$, $\frac{1}{3}$, and $\frac{1}{4}$ of their intensities in 1928. $H\epsilon$, $H\zeta$, $H\eta$, which were measured in 1928, cannot be detected in 1931. The nebular line, λ 3760, on the other hand, has approximately the same intensity on the two plates.

(3) The λ 4686 line of ionised helium is considerably weaker than in 1928 ($H\alpha$ being used as the basis of comparison). The reduction in its intensity is approximately in the same proportion as that of $H\beta$ but is not so great as that of $H\gamma$.

(4) The λ 6087 line, which in the latter part of 1928 had become the most prominent line in the spectrum with the exception of the He^+, λ 4686 line, is easily the strongest line in 1931.

(5) The λ 5722 line, which became prominent between the end of 1926 and early 1928, is still strong in 1931, having about the same intensity relative to $H\alpha$ as in 1928.

(6) The nebular lines, N_1, N_2, seem to be absent. The line measured at λ 4989 is definitely not N_1. By placing the 1931 plate film to film with the 1928 April plate, no trace can be seen on the former plate of a line in the position of N_1 on the latter plate. It follows that N_2 must also be absent, and the line measured at λ 4968 is probably the line which, in 1928, was blended with N_2, as mentioned above.

(7) In 1928, lines due to neutral helium were present, though somewhat weak. The strongest of the *He* lines were $\lambda\lambda$ 5876 and 4472. No trace of these lines can be seen in the 1931 spectrum.

In 1928, several lines of the Pickering series of ionised helium were present. In 1931, the λ 5411 line appears to be the only member of this series present, apart from the lines which may be blended with the Balmer series of hydrogen. It would appear, therefore, that the decrease in intensity towards the violet of the members of the Pickering series is more rapid in 1931 than in 1928. In this respect, the behaviour of this series is analogous to that of the Balmer series.

Of the other lines in the spectrum, the two at $\lambda\lambda$ 4121·2, 4714·3 agree closely in wave-length with helium lines belonging to the (1^3P°–m^3S) series. Neither helium line was present in 1928 but both were present in 1926, when they were greatly surpassed in intensity by the *He* lines, $\lambda\lambda$ 4472, 5876. Their identification with the neutral helium lines in the 1931 spectrum seems extremely doubtful. The λ 4121 line might at first sight be identified as the λ 4119·2 line due to ionised oxygen, which is present in the spectra of certain Wolf-Rayet stars. But the absence of other O^+ lines seems conclusive against this identification.

The weak line at λ 4620 was not detected in the 1928 spectrum. A weak line of this wave-length has been measured in the spectra of several Wolf-Rayet stars.

The weak line at λ 4899 is apparently identical with the lines measured at λ 4900 in 1926 and at λ 4894 in 1928. The stronger line at λ 4942 was present in 1928 but not in 1926; it is probably the same line that is measured at λ 4940 in the spectra of Wolf-Rayet stars. The line at λ 4989 is a weak line which was not observed in the spectrum previously; its origin is not known and it has not been observed in the spectra of Wolf-Rayet stars.

The pair of lines at $\lambda\lambda$ 5152, 5175 form a broad, ill-defined blend, the wave-lengths of which are somewhat uncertain. They are apparently the same lines as those measured in the 1928 spectrum at $\lambda\lambda$ 5149, 5173. The line at λ 5276 agrees reasonably well in position with a line observed in the 1926 and 1928 spectra. This line was attributed in 1926 mainly to the forbidden line of ionised iron at λ 5273; in 1928 it was probably due in part at least to the same origin. In 1931 it is appreciably stronger and must have some other origin. A line of approximately the same wave-length is occasionally observed in Wolf-Rayet spectra. The line at λ 5310 agrees closely with wave-lengths measured in the 1926 and 1928 spectra; its origin is not known.

In the fading from the spectrum of the typical nebular lines, N_1, N_2, Nova Pictoris has followed the course of several previous novæ. The disappearance of these lines from a nova spectrum was first observed

by Hartmann * in the case of Nova Persei, 1901. In the autumn of 1901, N_1 and N_2 were present in the spectrum of Nova Persei as weak bands. By the autumn of 1902, N_1 had become the strongest emission in the region of the spectrum under observation (which did not include the whole of the visual region): the He^+, λ 4686, line was the second strongest and then came N_2. Only traces of $H\beta$ and $H\gamma$ were visible. In the autumn of 1907, He^+, λ 4686, was the strongest line; the hydrogen lines, $H\beta$, $H\gamma$, and $H\delta$ were considerably weaker. The Wolf-Rayet line, λ 4618, was also present but no trace could be detected of N_1 or N_2.

In 1914, Adams and Pease † photographed the spectra of Nova Aurigæ, 1891; Nova Persei, 1901; Nova Lacertæ, 1910; and Nova Geminorum No. 2, 1912, with a small single-prism slit spectrograph at the primary focus of the 60-inch reflector. The N_1, N_2 lines were absent from the spectra of Nova Aurigæ and Nova Persei. The spectrum of the latter star was substantially as observed by Hartmann in 1907, and is thus probably to be considered as its permanent spectrum. The N_1, N_2 lines were fairly strong in the spectra of both Nova Lacertæ and Nova Geminorum. The observations of Hartmann and of Adams and Pease thus show that twenty-three years after the discovery of Nova Lacertæ, and six years after the discovery of Nova Persei, the N_1, N_2 lines had disappeared from the spectra. The observations do not indicate, however, at what date the lines disappeared. On the other hand, four years after the outbreak of Nova Lacertæ and two years after the outbreak of Nova Geminorum No. 2, the N_1, N_2 lines were still prominent in the spectra.

In the spectrum of Nova Pictoris the N_1, N_2 lines never became at all prominent, and their relatively early disappearance might have been anticipated. They were present in the spectrum at the beginning of 1929, nearly four years after the outbreak, but had disappeared early in 1931, nearly six years after the outbreak.

Hartmann and Adams and Pease remark upon the increase in the strength of the continuous spectrum after the disappearance of the N_1, N_2 bands and the general similarity of the spectrum to the spectra of Wolf-Rayet stars. It has been remarked above that the strength of the continuous spectrum of Nova Pictoris in 1931 is about equal to the strength in 1928. This estimate is relative to the intensity of $H\alpha$. If the spectrum in the visual region had not been available and $H\beta$, γ, δ had been used as a basis of comparison, it would have been concluded that the continuous spectrum had considerably strengthened between 1928 and 1931. If the intensity of the continuous spectrum is estimated relatively to the total intensity of the emission bands, then it must certainly be concluded that the continuous spectrum is appreciably stronger in 1931 than in 1928.

In its general appearance, the spectrum of Nova Pictoris in 1931 bears a strong resemblance to the spectra of Wolf-Rayet stars. In the photographic region, the continuous spectrum is relatively strong and the λ 4686, He^+, emission is the strongest line. The other lines in this

* *Ast. Nach.*, **177**, 113, 1908 (No. 4232).
† *Ap. J.*, **40**, 294, 1914; *Mt. Wilson Contributions*, No. 87.

region are generally weak, and include several which have been observed in the spectra of one or more Wolf-Rayet stars. The three strong emissions in the visual region, *Hα* and λλ 5723, 6088, form the most conspicuous feature of the spectrum. It is probable that the permanent spectrum of Nova Pictoris will not differ greatly from the spectrum as photographed in 1931 February.

Note on the Spectrum of η Carinæ.

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The spectrum of the variable star η Carinæ has been discussed by P. W. Merrill,* who identified most of the lines in the spectrum, whose origin had previously been unknown, as due to "forbidden" lines of the ionised iron spectrum. Merrill utilised measures of the spectrum by Lunt and by Moore and Sanford, which extended from λ 4068 to λ 5169. In connection with a discussion of the spectrum of Nova Pictoris, in which at a certain stage emission bands are present corresponding in position with many of the bright lines in the spectrum of η Carinæ due to forbidden transitions in the ionised iron atom, it became of interest to examine the spectrum of η Carinæ over a wider range of wave-lengths. For this purpose a spectrum obtained at the Union Observatory, Johannesburg, with the 16° objective prism in conjunction with the Cooke 6-inch triplet objective of the Franklin-Adams telescope, was kindly lent to me by Mr. H. E. Wood, Union Astronomer. This spectrum was obtained on 1919 April 30 and is measurable from λ 3939 to *Hα*.

The derived wave-lengths, estimated intensities on an arbitrary scale, suggested identifications, and multiplet designations are given in Table I.

Table I.

Bright Lines in the Spectrum of η Carinæ.

Measured λ.	Int.	Identification.	Tabular λ.	Obs.—Tab.	Multiplet Designation.	Remarks.
3938·5	1	?[*Fe*$^+$]	3937·8	+0·7	$a^4G_{2\frac{1}{2}}$–$a^6D_{2\frac{1}{2}}$	
		Fe$^+$	3939·1	−0·6		
3970·9	2	*Hε*	3970·1	+0·8	2^2P°–7^2D	
3992·9	1	?[*Fe*$^+$]	3991·9	+1·0	$a^4G_{5\frac{1}{2}}$–$a^6D_{3\frac{1}{2}}$	
4068·7	3	Neb.	4068·6	+0·1		
4101·6	6	*Hδ*	4101·7	−0·1	2^2P°–6^2D	
4113·6	1					
4121·2	1	*Fe*$^+$	4122·9	−1·7	$b^4P_{2\frac{1}{2}}$–$z^4F^\circ_{2\frac{1}{2}}$	
4127·6	0	*Fe*$^+$	4128·7	−1·1	$b^4P_{2\frac{1}{2}}$–$z^4D^\circ_{2\frac{1}{2}}$	
4173·5	3	*Fe*$^+$	4173·5	0·0	$b^4P_{2\frac{1}{2}}$–$z^4D^\circ_{2\frac{1}{2}}$	

* *Ap. J.*, **67**, 391, 1928; *Mt. Wilson Contributions*, No. 354.

TABLE I.—*continued.*

Measured λ.	Int.	Identification.	Tabular λ.	Obs.–Tab.	Multiplet Designation.	Remarks.
4178·2	6	[Fe+]	4177·2	+1·0	$a^4G_{4\frac{1}{2}}$–$a^4F_{4\frac{1}{2}}$	
		Fe+	78·9	−0·7	$b^4P_{2\frac{1}{2}}$–$z^4F°_{3\frac{1}{2}}$	
4201·3	1	..	..	..	..	Uncertain.
4233·0	10	Fe+	4233·2	−0·2	$b^4P_{2\frac{1}{2}}$–$z^4D°_{3\frac{1}{2}}$	
4244·2	15	[Fe+]	4244·0	+0·2	$a^4G_{5\frac{1}{2}}$–$a^4F_{4\frac{1}{2}}$	
		[Fe+]	44·8	−0·6	$a^4G_{3\frac{1}{2}}$–$a^4F_{3\frac{1}{2}}$	
4259·3	1	Fe+	4258·2	+1·1	$b^4P_{1\frac{1}{2}}$–$z^4F°_{1\frac{1}{2}}$	
4276·9	8	[Fe+]	4276·9	0·0	$a^4G_{4\frac{1}{2}}$–$a^4F_{3\frac{1}{2}}$	
4287·4	20	[Fe+]	4287·4	0·0	$a^6D_{4\frac{1}{2}}$–$a^6S_{2\frac{1}{2}}$	
4290·8	2	Ti+	4290·2	+0·6	$a^4P_{1\frac{1}{2}}$–$z^4D°_{2\frac{1}{2}}$	
4298·1	2	Fe+	4296·6	+1·5	$b^4P_{1\frac{1}{2}}$–$z^4F°_{2\frac{1}{2}}$	Broad blend.
4299·5	1	Ti+	4300·1	−0·6	$a^4P_{2\frac{1}{2}}$–$z^4D°_{3\frac{1}{2}}$	
4304·2	4	Fe+	4303·2	+1·0	$b^4P_{1\frac{1}{2}}$–$z^4D°_{1\frac{1}{2}}$	
		[Fe+]	05·9	−1·7	$a^4G_{2\frac{1}{2}}$–$a^4F_{2\frac{1}{2}}$	
4320·0	4	[Fe+]	4319·6	+0·4	$a^4G_{3\frac{1}{2}}$–$a^4F_{2\frac{1}{2}}$	
4341·0	15	Hγ	4340·5	+0·5	$2^2P°$–5^2D	
4347·8	1	[Fe+]	4346·9	+0·9	$a^4G_{5\frac{1}{2}}$–$a^4F_{3\frac{1}{2}}$	
4352·5	8	Fe+	4351·8	+0·7	$b^4P_{1\frac{1}{2}}$–$z^4D°_{2\frac{1}{2}}$	
		[Fe+]	52·8	−0·3	$a^4G_{4\frac{1}{2}}$–$a^4F_{2\frac{1}{2}}$	
4359·1	20	[Fe+]	4358·4	+0·7	$a^4G_{2\frac{1}{2}}$–$a^4F_{1\frac{1}{2}}$	
		[Fe+]	59·3	−0·2	$a^6D_{3\frac{1}{2}}$–$a^6S_{2\frac{1}{2}}$	
4364·2	2	..	..	..	..	Doubtful.
4372·6	3	[Fe+]	4372·5	+0·1	$a^4G_{3\frac{1}{2}}$–$a^4F_{1\frac{1}{2}}$	
4385·2	4	[Fe+]	4382·8	+2·4	$b^4F_{3\frac{1}{2}}$–$a^6D_{4\frac{1}{2}}$	
		Fe+	85·4	−0·2	$b^4P_{1\frac{1}{2}}$–$z^4D°_{1\frac{1}{2}}$	
4395·1	1	Ti+	4394·1	+1·0	$a^2P_{1\frac{1}{2}}$–$z^4D°_{1\frac{1}{2}}$	
		Ti+	95·0	+0·1	$a^2D_{2\frac{1}{2}}$–$z^2F°_{3\frac{1}{2}}$	
		Ti+	95·8	−0·7	$b^4P_{2\frac{1}{2}}$–$z^4D°_{3\frac{1}{2}}$	
4415·9	20	[Fe+]	4413·8	+2·1	$a^6S_{2\frac{1}{2}}$–$a^6D_{2\frac{1}{2}}$	
		[Fe+]	16·3	−0·4	$b^4F_{4\frac{1}{2}}$–$a^6D_{4\frac{1}{2}}$	
		Fe+	16·8	−0·9	$b^4P_{1\frac{1}{2}}$–$z^4D°_{1\frac{1}{2}}$	
4452·2	8	[Fe+]	4452·1	+0·1	$a^6S_{2\frac{1}{2}}$–$a^6D_{1\frac{1}{2}}$	Blended.
4457·7	6	[Fe+]	4458·0	−0·3	$b^4F_{3\frac{1}{2}}$–$a^6D_{3\frac{1}{2}}$	
4473·5	4	Fe+	4472·9	+0·6	$b^4F_{2\frac{1}{2}}$–$z^4F°_{1\frac{1}{2}}$	
		[Fe+]	74·9	−1·4	$a^6S_{2\frac{1}{2}}$–$a^6D_{\frac{1}{2}}$	
4491·1	8	[Fe+]	4488·8	+2·3	$b^4F_{2\frac{1}{2}}$–$a^6D_{2\frac{1}{2}}$	
		Fe+	89·2	+1·9	$b^4F_{3\frac{1}{2}}$–$z^4F°_{2\frac{1}{2}}$	
		Fe+	91·4	−0·3	$b^4F_{1\frac{1}{2}}$–$z^4F°_{1\frac{1}{2}}$	
		[Fe+]	92·6	−1·5	$b^4F_{4\frac{1}{2}}$–$a^6D_{3\frac{1}{2}}$	

TABLE I.—*continued.*

Measured λ.	Int.	Identification.	Tabular λ.	Obs.—Tab.	Multiplet Designation.	Remarks.
4509·2	5	*Fe*$^+$	4508·3	+0·9	$b^4F_{1\frac{1}{2}}$–$z^4D^\circ_{\frac{1}{2}}$	Broad blend.
4516·8	5	*Fe*$^+$	4515·3	+1·5	$b^4F_{2\frac{1}{2}}$–$z^4F^\circ_{2\frac{1}{2}}$	
4522·1	8	*Fe*$^+$	4520·2	+1·9	$b^4F_{4\frac{1}{2}}$–$z^4F^\circ_{3\frac{1}{2}}$	
		Fe$^+$	22·6	−0·5	$b^4F_{2\frac{1}{2}}$–$z^4D^\circ_{1\frac{1}{2}}$	
4550·5	8	*Fe*$^+$	4549·5	+1·0	$b^4F_{3\frac{1}{2}}$–$z^4D^\circ_{2\frac{1}{2}}$	Blended.
		Ti$^+$	49·6	+0·9	$a^2H_{5\frac{1}{2}}$–$z^2G^\circ_{4\frac{1}{2}}$	
4555·6	4	*Fe*$^+$	4555·9	−0·3	$b^4F_{3\frac{1}{2}}$–$z^4F^\circ_{3\frac{1}{2}}$	
4559·5	3	*Cr*$^+$	4558·7	+0·8	$b^4F_{4\frac{1}{2}}$–$z^4D^\circ_{3\frac{1}{2}}$	
4583·8	15	*Fe*$^+$	4583·8	0·0	$b^4F_{2\frac{1}{2}}$–$z^4F^\circ_{3\frac{1}{2}}$	
4588·4	4	*Cr*$^+$	4588·2	+0·2	$b^4F_{3\frac{1}{2}}$–$z^4D^\circ_{2\frac{1}{2}}$	Overlapping preceding.
4619·6	1	*Cr*$^+$	4616·7	+2·9	$b^4F_{1\frac{1}{2}}$–$z^4D^\circ_{1\frac{1}{2}}$	
		Cr$^+$	18·9	+0·7	$b^4F_{2\frac{1}{2}}$–$z^4D^\circ_{1\frac{1}{2}}$	
		Fe$^+$	20·5	−0·9	$b^4F_{3\frac{1}{2}}$–$z^4D^\circ_{3\frac{1}{2}}$	
4630·0	6	*Fe*$^+$	4629·3	+0·7	$b^4F_{4\frac{1}{2}}$–$z^4F^\circ_{4\frac{1}{2}}$	
4638·9	2	[*Fe*$^+$]	4639·7	−0·8	$b^4P_{\frac{1}{2}}$ –$a^6D_{1\frac{1}{2}}$	
4665·8	1	*Fe*$^+$	4663·7	+2·1	$a^6S_{2\frac{1}{2}}$–$z^4F^\circ_{3\frac{1}{2}}$	[*Fe*$^+$], 4661·8, $a^6S_{2\frac{1}{2}}$–$a^4F_{4\frac{1}{2}}$ possibly involved.
		Fe$^+$	66·8	−1·0	$b^4F_{3\frac{1}{2}}$–$z^4F^\circ_{4\frac{1}{2}}$	
4728·8	6	[*Fe*$^+$]	4728·1	+0·7	$b^4P_{1\frac{1}{2}}$—$a^6D_{2\frac{1}{2}}$	
		Fe$^+$	31·5	−2·7	$a^6S_{2\frac{1}{2}}$–$z^4D^\circ_{3\frac{1}{2}}$	
4747·3	0					
4774·7	2	[*Fe*$^+$]	4774·7	+0·0	$b^4F_{3\frac{1}{2}}$–$a^4F_{4\frac{1}{2}}$	
4815·4	5	[*Fe*$^+$]	4814·6	+0·8	$b^4F_{4\frac{1}{2}}$–$a^4F_{4\frac{1}{2}}$	
4861·7	40	Hβ	4861·3	+0·4	2^2P°–4^2D	
4873·7	2	[*Fe*$^+$]	4874·4	−0·7	$b^4P_{2\frac{1}{2}}$–$a^4F_{3\frac{1}{2}}$	
4889·9	4	[*Fe*$^+$]	4889·7	+0·2	$b^4P_{2\frac{1}{2}}$–$a^6D_{3\frac{1}{2}}$	
4923·6	15	*Fe*$^+$	4923·9	−0·3	$a^6S_{2\frac{1}{2}}$–$z^6P^\circ_{1\frac{1}{2}}$	
4948·7	0	[*Fe*$^+$]	4947·4	+1·3	$b^4F_{4\frac{1}{2}}$–$a^4F_{3\frac{1}{2}}$	
5018·9	15	*Fe*$^+$	5018·4	+0·5	$a^6S_{2\frac{1}{2}}$–$z^6P^\circ_{2\frac{1}{2}}$	
5159·5	5	[*Fe*$^+$]	5158·1	+1·4	$b^4P_{1\frac{1}{2}}$–$a^4F_{3\frac{1}{2}}$	Broad blend.
5168·5	12	*Fe*$^+$	5169·0	−0·5	$a^6S_{2\frac{1}{2}}$–$z^6P^\circ_{3\frac{1}{2}}$	
5234·9	1	*Fe*$^+$	5234·6	+0·3	$a^4G_{3\frac{1}{2}}$–$z^4F^\circ_{2\frac{1}{2}}$	
5263·5	2	*Fe*$^+$	5264·8	−1·3	$a^4G_{2\frac{1}{2}}$–$z^4D^\circ_{1\frac{1}{2}}$	Blended.
5274·9	5	[*Fe*$^+$]	5273·4	+1·5	$b^4P_{2\frac{1}{2}}$–$a^4F_{4\frac{1}{2}}$	
		Fe$^+$	76·0	−1·1	$a^4G_{4\frac{1}{2}}$–$z^4F^\circ_{3\frac{1}{2}}$	
5317·2	3	*Fe*$^+$	5316·6	+0·6	$a^4G_{5\frac{1}{2}}$–$z^4F^\circ_{4\frac{1}{2}}$	
		Fe$^+$	16·8	+0·4	$a^4G_{3\frac{1}{2}}$–$z^4D^\circ_{2\frac{1}{2}}$	
5337·6	2	*Fe*$^+$	5337·7	−0·1	$a^4G_{2\frac{1}{2}}$–$z^4D^\circ_{2\frac{1}{2}}$	
		Cr$^+$	37·8	−0·2	$b^4F_{2\frac{1}{2}}$–$z^4F^\circ_{1\frac{1}{2}}$	
5374·3	0	..	..	..	..	Suspected.
5873·2	0	? *He*	5875·7	−2·5	1^3P°–2^3D	,,
6562·4	80	*Hα*	6562·8	−0·4	2^2P°–3^2D	

The lines attributed to transitions from metastable states include 5 lines due to transitions from a^6S to a^6D levels; 3 lines due to transitions from b^4P to a^6D levels; 5 lines due to transitions from b^4F to a^6D levels; 4 lines due to transitions from b^4F to a^4F levels; 10 lines due to transitions from a^4G to a^4F levels; and 2 lines due to transitions from b^4P to a^4F levels. The corresponding terms are all even. As pointed out by Merrill, the final state of these transitions is one or other of the two lowest terms, a^6D and a^4F, of the ionised atom of iron and the initial state is in every case the final state of one or more strong Fe^+ lines.

In the region to the red of λ 5000, the spectrum contains two strong lines of the Fe^+ multiplet (a^6S–z^6P°), two forbidden lines of the multiplet (b^4P–a^4F), and several weaker lines, mainly due to Fe^+. The region between λ 5338 and $H\alpha$ contains few lines, and such as are present are extremely weak. $H\alpha$ is very strong and the intensities of the lines of the Balmer series decrease very rapidly towards the violet.

On the Relationship between Solar Prominences and the Forms of the Corona. By William J. S. Lockyer, M.A., Ph.D., Major (late R.A.F.). (Plate 13.)

In June of the year 1903 I communicated to the Society my first paper * relating to the probable connection between the latitudes of solar prominences and the three typical forms of the solar corona. The data for this paper covered the period from 1870 to 1900. A second paper † was communicated in 1922 April, and this brought the data discussed up to the year 1919, the same conclusion being again reached, namely that (p. 329) "the various forms of the corona are closely associated with the active zones of prominences."

Twelve years, or about one sunspot period, have now passed, and seven total eclipses have been observed during this interval. Further, some important contributions have been made on the subject of the cause of the variation in the shape of the corona. It was therefore considered opportune to bring together in one diagram all the data I have gathered on which have been based the conclusions I have previously drawn, adding the new observations of both prominence activity and latitude and the observed forms of the seven coronas.

The sources of the data employed were given in detail in the two previous papers, but they may be briefly summarised here. The sunspot data, including both mean daily areas and latitudes, are based on the Greenwich Publications. The frequencies and latitudes of the prominences are from the observations of Respighi, Secchi and Tacchini, Ricco and Mascari, Evershed, and, later, from the Kodaikanal Publications. No detailed information was given in the first paper as to the sources of the information regarding the different forms of the corona observed, but all available publications were searched. The second

* *M.N.*, **63**, 481, 1903. † *Ibid.*, **82**, 323, 1922.

paper, however, contained references in detail for each eclipse, which included the forms noted from 1893 to the 1919 eclipse.

The following references include similar information for the succeeding eclipses up to the year 1930.

Forms of the Corona from 1922 *to* 1930.

Year.	Month.	Reference and Authority.	Form of Corona.
1922	Sept. 21	P.A.S.P., **35**, 38, 1923. W. W. Campbell.	"Eclipse occurred evidently with the sun in a remarkably quiescent state."
,,	,,	P.A.S.P., **35**, 61, 1923. J. H. Moore.	"Type corresponding to sunspot minimum."
,,	,,	M.N.R.A.S., **84**, 155, 1924. G. F. Dodwell and C. R. Davidson.	"The corona was of a minimum sunspot type."
,,	,,	"Eclipses of the Sun," 337, 1923. S. A. Mitchell.	"The corona of 1922 was of the sunspot minimum type."
1923	Sept. 10	A.J., **61**, 78, 1925. J. A. Miller and R. W. Marriott.	"The general type of the corona is characteristic of that of sunspot minimum."
,,	,,	J.R.A.S.Can., **17**, 1923, pl. ii. C. A. Chant.	The published photographs exhibit the "equatorial" type of corona (W.J.S.L.).
,,	,,	Nature, **112**, 805. Astro. Notes from J. Worthington's photographs.	"The outline conforms to the type of sunspot minimum."
1925	Jan. 24	Handbuch der Astrophysik, **4**, 263, 1929. S. A. Mitchell.	"In general shape the corona corresponded closely with that expected from condition of the sun near spot minimum."
,,	,,	P.A.S.P., **36**, 102, 1925.	"Professor Slocum reports the form of the corona to have been of the 'intermediate' type."
,,	,,	In a letter to W. J. S. Lockyer from J. Worthington.	"As you will see, the corona is not of pure minimum type."
1926	Jan. 14	Handbuch der Astrophysik, **4**, 318, 1929. S. A. Mitchell.	"Closely resembled that associated with maximum of spots."
,,	,,	The Observatory, **49**, 64, 1926.	"Professor Miller's telegram reports that the corona was of the maximum type."
,,	,,	Mem. R.A.S., **64**, pt. iv. 143, 1927. C. R. Davidson and F. J. M. Stratton.	"The corona is of intermediate type with streamers radiating at all angles. The polar plumes are absent."
,,	,,	P. dell. O.A. della Univ. Bol., **1**, No. 8, 1926. G. Horn-D'Arturo.	No statement made but several illustrations exhibit the "polar" form (W.J.S.L.).
1927	June 29	M.N.R.A.S., **87**, 664, 1927. Sir Frank Dyson.	"The corona is of maximum type."
,,	,,	M.N.R.A.S., **87**, 661, 1927. M. de Saussure.	"Noted its type as nearly corresponding to maximum of sunspots."
,,	,,	Nature, **120**, 52, 1927. W. H. Steavenson.	"The general appearance was that of a typical maximum corona."
,,	,,	Handbuch der Astrophysik, **4**, 318, 1929. S. A. Mitchell.	"The corona was distinctly that of maximum type."

Forms of the Corona from 1922 *to* 1930—*continued.*

Year.	Month.	Reference and Authority.	Form of Corona.
1929	May 9	Nature, **124**, 90, 1929. J. Jackson.	"The corona is of intermediate form, and shows considerable difference from that generally obtained at sunspot maximum."
,,	,,	Popular Astronomy, **37**, 504, 1929. J. A. Miller and R. W. Marriott.	"It is clearly not of the maximum type, and it lacks the polar rays and long equatorial streams of the minimum type."
,,	,,	B.A. Ist. Kijoto. Imp. Univ., Japan, No. 155, 1929. Professor Yamamoto.	"Intermediate type, polar streamers are weak."
1930	Oct. 21	Daily Science News Bulletin, Oct. 24, 1930; Nature, **126**, 785, 1930. S. A. Mitchell.	"The corona was of a type intermediate between maximum and minimum."

It may be as well to define the three main types into which the forms of the corona have been divided in my previous papers, and which have been continued in use in this communication. They are as follows:—

The "polar" type includes those irregular forms in which the streamers are situated all round the solar disc; the polar "rifts" in this type are conspicuous by their absence. This type is usually termed the "maximum" or "maximum sunspot" type.

The "equatorial" type is exhibited when the streamers are restricted to comparatively low solar latitudes, and the poles display the "polar rifts" to the best advantage and have a large spread in latitude. This type is sometimes termed the "minimum" or "minimum sunspot" type, and also the "wind-vane" or "fish-tail" form.

The "intermediate" type displays the streamers in mid-solar latitudes. The "polar rifts" are present, but not so extensive in latitude as in the "equatorial" type. The streamers also approach nearer the polar regions than in the "equatorial" type, while the equatorial extensions are not in such great evidence. This type is sometimes referred to as the "square corona," and "intermediate" between sunspot maximum and minimum type.

The forms of the corona from the year 1922 to 1930, as gathered from the references given above, may be defined as follows:—

1922	Sept. 21	Equatorial (Minimum).
1923	Sept. 10	Equatorial (Minimum).
1925	Jan. 24	Equatorial to Intermediate.
1926	Jan. 14	Polar (Maximum).
1927	June 29	Polar (Maximum).
1929	May 9	Intermediate.
1930	Oct. 21	Intermediate.

In five out of the seven cases the general opinion as to the type of corona observed seems to be fairly consistent, but in the other two

cases, namely, the coronas of 1925 and 1926, there seems to be some doubt.

In the case of the former some beautiful contact positives on thick film sent to me by Mr. Worthington give the impression of a typical "fish-tail" or "equatorial" form. This opinion, coupled with those suggested among the references given above, suggests that a compromise should be made, and the type has thus been classified as one between "intermediate" and "equatorial."

For the 1926 corona, both Mitchell and Miller describe the form as of "maximum" type, and the reproductions of the photographs secured by Horn-D'Arturo exhibit, in my opinion, the "polar" form. On the other hand, Davidson and Stratton describe the form as "intermediate," but yet state that there were "streamers radiating at all angles," and that "the polar plumes are absent," both of which features are indicative of the "polar" form. The type here adopted has been the "polar" one.

These data and those relating to the sunspots and prominences, as far up to date as published values exist, are all plotted in fig. 1. The sunspot curve is at the top, and is then followed by the prominence curve, which shows the mean daily areas for each year. Next follow the sunspot latitude curves. These curves represent the mean heliographic latitude for each hemisphere of the spotted areas for each year. It must be remembered, however, that individual sunspots are very restricted in latitude, very seldom occurring above 40° or below 5°. When the sun is most active, as regards sunspots, their latitude is only about 15° north and south. These latitude curves thus represent the movements of the centres of the sunspot zones from year to year. The prominence latitude curves are formed by determining for each year the latitudes of the peaks of the curve representing the distribution of prominence activity from the poles to the equator. Thus these curves represent the changes in the latitudes of the centres of the main prominence zones year by year.

The formation of both the sunspot and prominence latitude curves may best be understood by means of the diagram shown in fig. 2. This displays, for each of the three years 1892, 1893, and 1894, the distribution in latitude of prominence areas (dark areas) and spotted areas (light areas). While the scale for latitude is the same for both phenomena, those for the prominence and spotted areas are not the same, although in the three figures the areas for each phenomenon are drawn on the same scale.

This figure brings out well the great difference between the latitude distribution of spots and that of prominences. While the year 1893 was a year of sunspot maximum, it will be seen how small is the distribution in latitude of the spotted area, and how extensive is the prominence distribution, reaching nearly to the poles in these years. It is the latitudes of the main peaks of the prominence curves that are recorded in fig. 1, and, as will be seen from fig. 2, their variation in both intensity and latitude is very marked.

Finally, below these prominence curves in fig. 1 are shown the

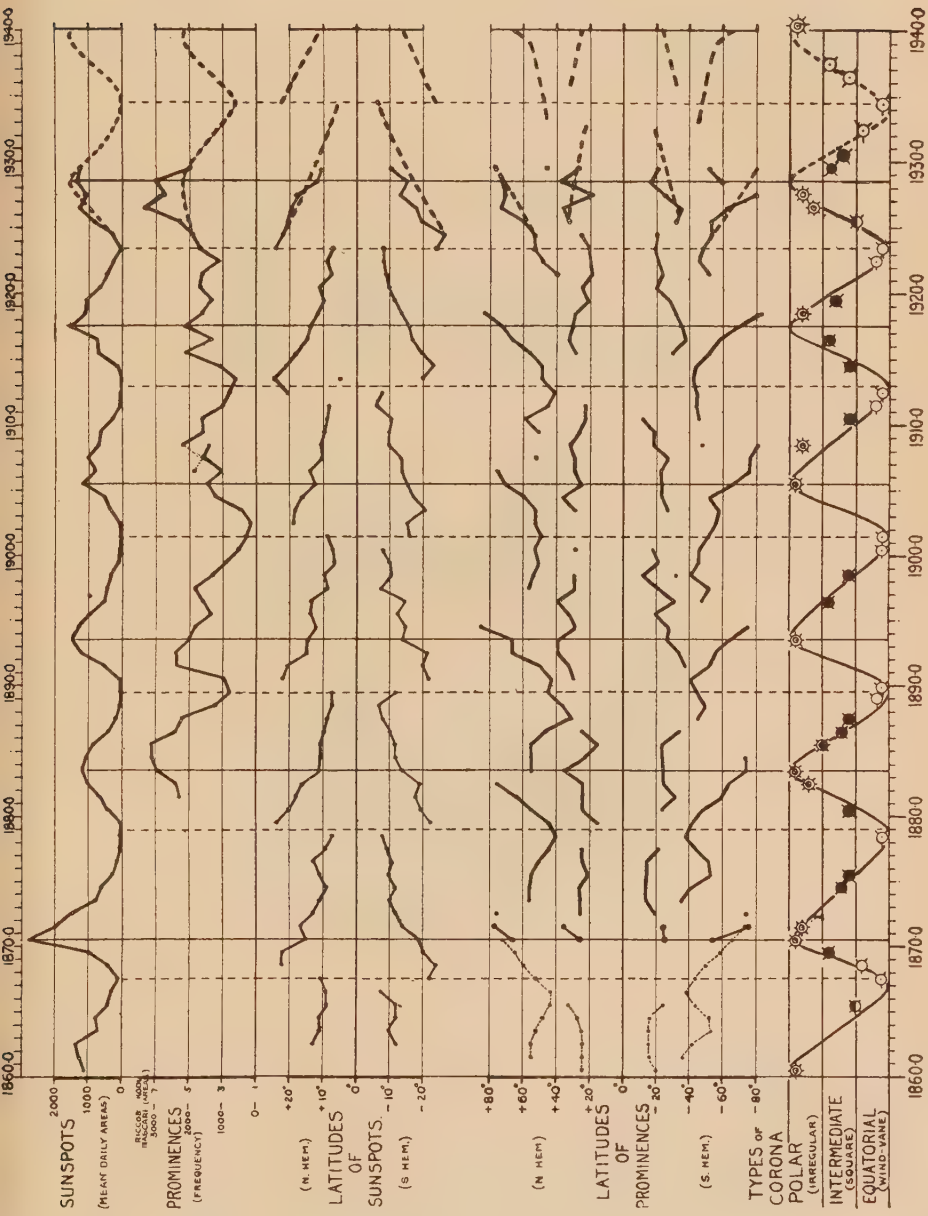
SUNSPOTS
(MEAN DAILY AREAS)
2000
1000
0
RIGOR (GCM.)
MAGGINI (MEAN)
3000 – 7
PROMINENCES
(FREQUENCY)
2000 – 5
1000 – 3
0 – 1
LATITUDES OF SUNSPOTS.
(N. HEM.)
+20°
+10°
0°
(S. HEM.)
– 10°
– 20°
LATITUDES OF PROMINENCES
(N HEM)
+80°
+60°
+40°
+20°
0°
(S. HEM.)
– 20°
– 40°
– 60°
– 80°
TYPES OF CORONA
POLAR
(IRREGULAR)
INTERMEDIATE
(SQUARE)
EQUATORIAL
(WIND-VANE)
1860·0
1870·0
1880·0
1890·0
1900·0
1910·0
1920·0
1930·0
1940·0

Fig. 1.

forms of the corona as observed at each eclipse. These are arranged in three horizontal strips, the uppermost one containing the " polar " forms, the middle the " intermediate," and the lowest the " equatorial " forms. Each eclipse is placed in the year of its occurrence according to the time scale given at the top and bottom of the diagram. The

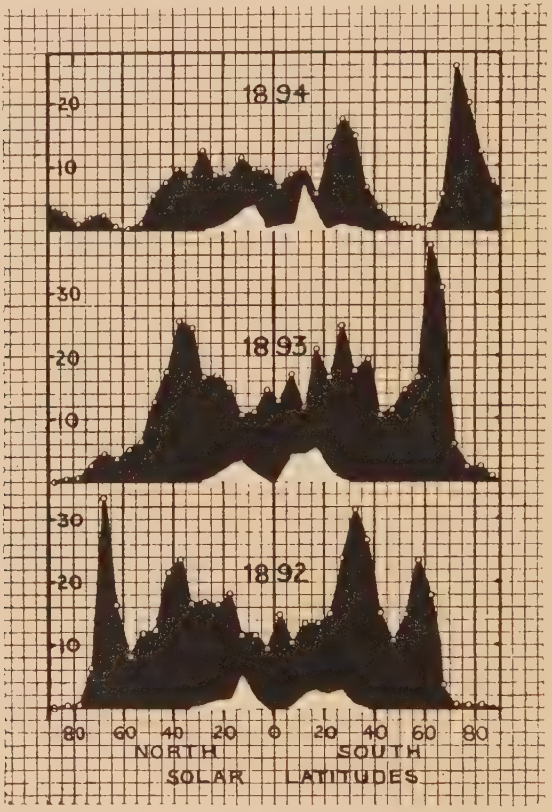

FIG. 2.

broken and continuous vertical lines represent the epochs of sunspot minimum and maximum respectively.

The chief points to be noted from the data recorded in fig. 1 are as follows :—

The sunspot and prominence curves show approximately similar epochs of maximum and minimum.

The curves of the latitudes of sunspots exhibit only one main zone in each hemisphere, the latitude of which decreases from about 24° at about sunspot minimum to about 6° at the following sunspot minimum, when it disappears entirely and a new zone commences again in the

higher latitude. At sunspot maximum the zone is situated in about latitude 15°.

In the case of the prominence curves there are nearly always two main zones present in each hemisphere. That which may be termed the "low-latitude zone" commences about two to three years after sunspot minimum, tends to move (like the spot zone) to lower latitudes, and disappears generally about two years before the next sunspot minimum. The other zone, which may be called the "high-latitude zone," commences about three years after sunspot maximum, and moves towards the poles in each hemisphere, terminating generally very abruptly close to the poles about a year after sunspot maximum. At about the sunspot minimum there is usually a period of about two or three years when only one prominence zone is in operation, and that is the "high-latitude zone," which is then situated in about latitude 45° in each hemisphere.

When the sun is most active, as exhibited by either spots or prominences, the latitudes of the prominences exceed 60° in both hemispheres *in every case*, except in the year 1928, when it was at 60° in the southern hemisphere only. Attention should specially be drawn to the fact that the *highest* prominence latitudes do not necessarily occur *at* the sunspot maxima, but more generally about a year later, and in one instance, namely in 1908, the highest latitude was attained in the southern hemisphere *three years* after the sunspot maximum.

Thus it is found that, when the prominences exceed 60° in latitude in either or both hemispheres, the corona is of the "polar" type in every case. This clearly shows without any doubt a very close relationship between "polar" coronas and high-latitude prominences.

The year 1908 is of particular interest in this respect. The type of corona recorded in that year was undoubtedly "polar," but it occurred two years after the sunspot maximum in 1905, the eclipse having taken place in January. The reason for its "polar" form was the fact that there existed, in the sun's southern hemisphere, prominences in latitude 80° (see fig. 1). This eclipse is of further interest because Evershed,* in 1917, in a discussion of his prominence observations in connection with the types of corona over a series of years, found that my theory was confirmed with the exception of this particular 1908 corona. At a later date,† however, he withdrew the statement about this exception, because he found that the orientation of the cardinal points, made by Mr. W. H. Wesley from Sir Francis McClean's original photographs, was incorrect, the north and south poles of the sun having been reversed.

The way in which Evershed came to detect this error is of interest, and may be briefly stated as follows. His Kodaikanal observations of prominences showed that, for the average conditions over each of the years 1907 and 1908, there was considerable activity over the sun's south polar region, but an almost complete absence of prominences over the north pole. As the eclipse occurred on 1908 January 3, his photographs taken early in January, and one taken seven hours after

* *Mem. Kod. Obs.*, **1**, part ii., 65, 1917. † *M.N.*, **83**, 154, 1923.

the eclipse itself, did not show any temporary departure from this distribution. An examination of Wesley's drawing from McClean's photographs displayed the opposite distribution as regards the coronal streamers, which in the drawing are more conspicuous over the north polar region than over the south. In view of this, he was constrained to doubt whether the Flint Island plates were correctly oriented, as his previous research, mentioned above, had led him to believe that there was a close correspondence between the coronal streamers and the principal zones of prominence activity. In examining another photograph of the corona, taken by Dr. W. W. Campbell at the same time and place as McClean's photographs, he found independent evidence " that shows, I think conclusively, that the orientation is incorrect, the north and south points being interchanged." Evershed was thus led to conclude that the corona of 1908 " is not an exceptional case, but, on the contrary, adds weight to the evidence previously found for a close relationship between prominences and coronal streamers, as advocated by Major Lockyer in his recent and also his earlier paper."

With regard to the type of one of the coronas, recorded in my second paper on this subject, an alteration has been made here in consequence of more recent information. This relates to the corona of 1918. This was originally placed on the line between " intermediate " and " polar " to indicate its rather doubtful character. C. A. Chant described the form as " not quite maximum," but a more recent statement has been given by S. A. Mitchell,* who writes: " The corona was of the type associated with maximum of sunspots but with the polar streamers much more pronounced than were expected." While Mitchell uses the expression " polar streamers," it seems most probable that this is meant to refer to " polar rifts." The type of this corona is now described here as " polar," and it has been definitely placed in the " polar " series.

While the " polar " forms of the corona occur when the prominences are in high latitudes, the " equatorial " forms are recorded when the zone or zones of prominences are in their lowest latitudes. In general this form is exhibited when there is only one prominence zone in operation. In nearly every case the " intermediate " types are associated with two zones of prominences in each hemisphere.

According to the distribution of the prominences there seems little doubt that there is a difference of form between a corona occurring when the sun is increasing its activity and one when the sun's activity is on the decline.

It may be mentioned here that all the additional data, which have been used since the year 1919, satisfy in every way the relationship between the prominence latitudes and the coronal forms, as deduced from the observations prior to that year.

In the case of the eclipse 1926 January 14 its " polar " type is justified by the prominence distribution. It will be seen from fig. 1 that the latitude of the high prominence zones for the years 1925 and 1926 reached 60° and 70° respectively in the northern hemisphere,

* *Handbuch der Astrophysik*, **4**, 254, 1929.

while it only attained 52° and 62° for the same years in the southern hemisphere. While this eclipse occurred nearly two years before the epoch of the adopted sunspot maximum in 1928, Mitchell seemed surprised that such a form as "polar" should occur so early in the solar cycle, for he states:* "On January 14, 1926, the corona had lost all of the characteristics of the minimum shape of corona and closely resembled that associated with maximum of the spots, even though the time was nearly two years before expected maximum of spots. The corona of June 29, 1927, was distinctly that of maximum type." That the form was due to the presence of polar prominences and not to sunspot action readily explains the occurrence of this "polar type."

Attention may be drawn to Horn-D'Arturo's paper (*loc. cit.*, p. 225), in which he gives a diagram of the prominences recorded on the limb on January 12 at Meudon, on January 13 at Catania, and those shown on the photographs of the American, British, and Italian expeditions at the eclipse itself on January 14. This shows that in the north-west quadrant of the sun a large prominence exceeded 60° in latitude, and in the north-east quadrant another extended from 50° to 60° in latitude. In the southern hemisphere there was no prominence of any importance recorded near the pole in the south-east quadrant, but there was one in the south-west quadrant extending from latitude 35° to 50°. This prominence distribution thus explains the appearance of polar streamers near the north pole and their partial absence at the south pole as recorded on the photographs.

Some interesting facts can be gathered from a determination of the *mean latitudes* of the prominence zones at the times of the occurrence of the different types of the corona.

In the following table (Table I.) the mean latitudes of both the high- and low-latitude prominence zones for each hemisphere, as shown in fig. 1, are tabulated for each form of corona. The "intermediate" type is here divided into two forms, namely "intermediate up," when the sun is increasing in activity, and "intermediate down," when it is decreasing. The last column gives the number of eclipses involved in determining the mean values for each type.

Table I.

Corona Type.	Prominence Zones.						No. of Eclipses.
	High Latitude.			Low Latitude.			
	S.H.	N.H.	Mean.	S.H.	N.H.	Mean.	
	°	°	°	°	°	°	
Equatorial . .	45·5	44·4	44·9	20·0	22·5	21·2	9
Intermediate up .	51·5	58·5	55·0	30·6	30·6	30·6	4
Polar . .	71·6	70·5	71·0	26·5	29·5	28·0	10
Intermediate down .	46·8	51·7	49·2	20·7	25·0	22·8	9

It should be pointed out that the 1930 eclipse is not included, as there are as yet no published prominence data available. Secondly, in the case of the "equatorial" type, out of the nine eclipses used only four are associated with low-latitude prominence zones, thus the

* *Handbuch der Astrophysik*, **4**, 318, 1929.

values given under the low-latitude zone heading are the means of only four eclipses. The absence of this zone from most of the eclipses of this type indicates that its "form" is made up chiefly by the high-latitude zones in about latitude 45° of each hemisphere.

The table shows very clearly the very high average value of the latitude of the high-latitude zone for the "polar" type of corona. This value is fairly consistent for all the eclipses, and the means for the two hemispheres are very close.

Coming to the two types of "intermediate," for which there are only four of the "up" type but nine of the "down," the latitudes of both the high- and low-latitude zones are greater in the former than in the latter. Thus for the high-latitude zone the mean latitude for the "up" is nearly six degrees higher than that for the "down," and for the low-latitude zone the "up" exceeds the "down" by nearly eight degrees. This difference, which is in the same direction for both zones, seems to be sufficiently large to suggest that there may be a small change of type at the times of the rise and fall of solar activity.

It was considered of interest and value to examine, if possible, the distribution of the prominence areas through one complete and consecutive series of coronal types, to see how each of the observed recorded distributions actually compared with the main coronal types, and if there were any pronounced difference between the "intermediate up" and the "intermediate down." A good series of homogeneous data was found to be available in the Kodaikanal Observatory Bulletins, in which are published half-yearly curves showing the mean prominence areas for each five degrees of latitude in each hemisphere.

The data were selected from the years 1912, 1916, 1918, 1919, and 1923, since eclipses occurred in these years in which the coronal forms were recorded as of the equatorial, intermediate up, polar, intermediate down, and equatorial types respectively. These yearly mean prominence areas were then plotted for each ten degrees of latitude, as shown in fig. 3, for each of the five years, the same scale being used throughout; this scale has, however, no special relation to the areas of the dark half suns which are all of the same size. The sequence to be followed is from bottom left-hand side up to top right-hand side down.

It will be seen that the main features of the coronal types are all reflected in the prominence distribution in latitude, the high latitude zones moving towards the poles simultaneously with the greater area of prominence activity, and reaching the poles at the epoch of the "polar" type of corona. The "equatorial" type is conspicuous by having only one prominence zone in each hemisphere.

There is here a decided difference between the prominence distribution in the "intermediate up" and the "intermediate down," the latter, in this instance, conforming more to the equatorial type, disregarding the greater area of prominence activity there displayed.

The white arrows in each of the five half suns are the plotted positions of the latitudes of the high and low prominence zones extracted from Table I., and show the *mean* positions of these zones from all the

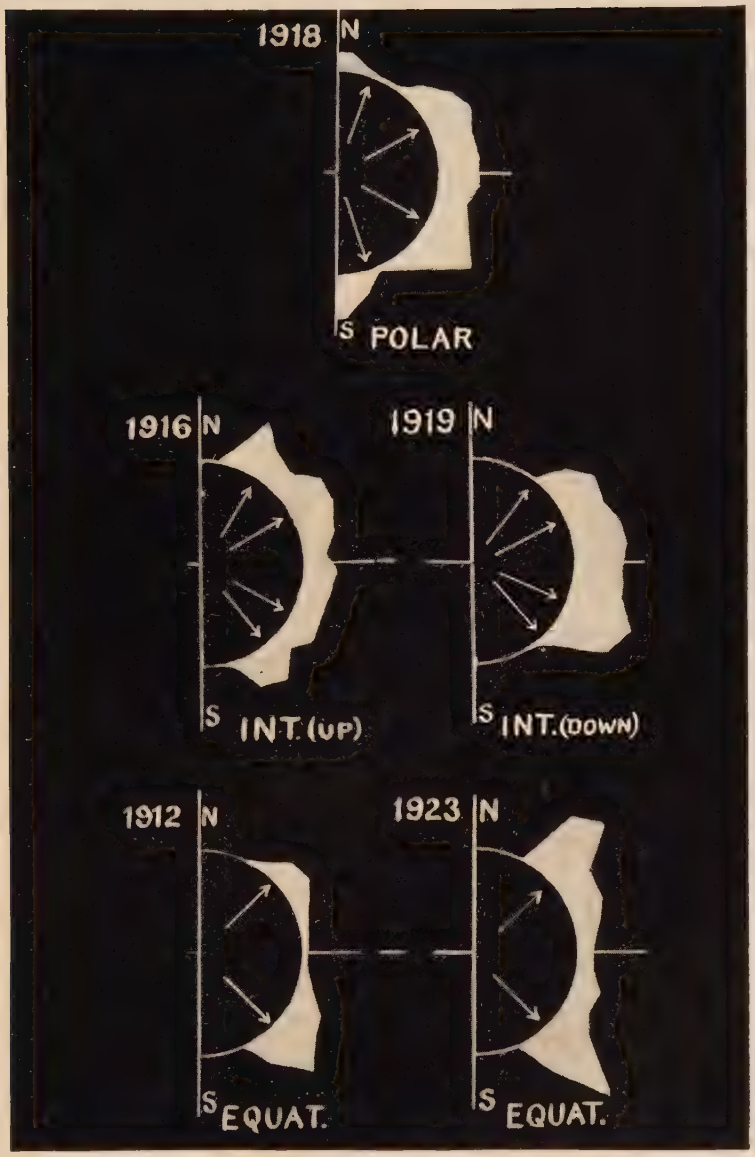
1918
N
S
POLAR
1916
N
S
INT. (UP)
1919
N
S
INT. (DOWN)
1912
N
S
EQUAT.
1923
N
S
EQUAT.

Fig. 3.

eclipses of each type. It will be seen that, for this single series of years, the arrows conform very well to the actual positions of the prominence zones.

Reference may now be made to the new contributions to the study of the causes of the changes of form of the corona, which have recently been published.

The first is due to Professor H. Ludendorff,* who in 1928 made tracings of nearly all the coronas between the years 1893 and 1927, drawing lines of equal intensity (isophotes) around the corona and marking at the same time the limb of the moon. As the shape of the corona approximates to that of an ellipse, the comparisons of the measures of the polar and equatorial diameters would give the means of determining the degree of ellipticity or flattening. Using the results of 154 of these comparisons he derived a formula for the flattening. He then looked for a correlation between the measured values giving the ellipticity of the corona and the phase in the eleven-year sunspot period, and found a relationship which indicated that the corona is nearly circular for eclipses near maximum of spots, but takes on a more and more flattened form after the sunspot maximum is passed. An exception to the general rule seemed to be the corona of 1918. Ludendorff thus concluded that the coronal forms were intimately connected with the frequency of sunspots.

Dr. S. A. Mitchell † expounds Ludendorff's theory under two separate references, and agrees with his view that the form of the corona depends on the spots that are actually on the sun at the time of the eclipse. He makes, however, this reservation, that "this does not mean that the spots themselves are the direct cause of changes in the radiation of the corona, but rather the underlying cause is the activity in the sun itself."

Professor Osten Bergstrand,‡ in his investigation in 1930 entitled "Sur les changements périodiques de la couronne solaire," analyses the data brought together and discussed by Ludendorff. He gives a correction to the values of the isophotes in the polar regions to allow for the superposition of streamers from equatorial or intermediate latitudes and deduces a quantity (p) which gives the ratio of the coronal luminosity in equatorial and polar regions at the same height above the solar surface. Bergstrand finds that there is a slight correlation between p and sunspot numbers, but the variations in the former follow much more closely those of prominences in the high-latitude zone. Thus his final conclusion is as follows (p. 18):—

"Le rapprochement avec les résultats que j'ai obtenu pour les variations du nombre coronal p donne l'irrésistible impression que le fort développement de la couronne polaire vers le temps du maximum des taches se rapporte à l'apparition simultanée des protubérances dans le voisinage des pôles du Soleil. De plus, le saut discontinu dans

* *Sitz. d. Preuss. Akad. d. Wiss.*, Phys.-Math. Klasse, **16**, 1928.

† *Pop. Ast.*, **37**, No. 4, 192, 1929; also *Handbuch der Astrophysik*, **4**, 338, etc., 1929.

‡ *Arkiv för Mat. Astr. och Fysik.*, K. Svenska Vetenskapsakademien, **22** A, No. 1, 1930.

la courbe des p paraît s'accorder avec la disparition subite des protubérances dans les hautes latitudes peu de temps après le maximum.

" Ainsi il semble que les changements périodiques de la couronne solaire, mis en lumière par les variations du nombre coronal p, offrent un fondement assez solide à l'hypothèse d'une dépendance de la couronne a l'egard des protubérances hypothèse énoncée déjà plus tôt par M. Lockyer."

A point of further interest in relation to this result is contained in a communication to me from Professor Bergstrand on 1930 February 4. In this he wrote :

" Je dois avouer qu'en calculant les valeurs du ' nombre coronal ' p, je n'avais point connaissance de votre théorie sur la liaison de la couronne avec les protubérances. Ainsi j'était tout libre de toute sorte d'idée préconçue à l'égard du résultat de mes calculs. A plus forte raison je fus frappé par l'accord de ce résultat avec vos idées concernant la liaison de la couronne et des protubérances. Je suis heureux d'avoir pu par mon recherche confirmer votre belle théorie."

Thus it will be seen that while Ludendorff and Mitchell both favour the action of sunspots, Evershed and Bergstrand adopt my view that it is the prominences which are responsible for the changes of the form of the corona.

It will be noticed in fig. 1 that from the year 1925 all the curves have been continued up to the year 1940 by broken lines. These were inserted in 1925 to attempt to forecast the changes in the different solar phenomena from year to year. Comparing those curves with those now available up to the year 1929 it will be seen that they conform fairly well with the actual facts. It was at that time considered probable that the high-latitude prominence zone would be still in evidence in both hemispheres in the year 1929 in about latitude 80°, and would therefore give rise to a " polar ' type of corona for that year. The latitude of this zone was, however, not so accommodating but only actually reached 52° in the southern hemisphere and 44° in the northern hemisphere, so that an " intermediate " type of corona should have been expected and was actually recorded.

In the case of the next few total solar eclipses, following that of 1930, namely those for the years 1932, 1934, 1936, 1937, and 1940, the forecasted curves suggest that the 1932 corona may be of the " equatorial " or on the boundary between an " intermediate " and " equatorial " type : that the 1934 corona will be of the pure " equatorial " type ; that the coronas of 1936 and 1937 should display an " intermediate " type ; and, finally, that the 1940 corona should be of the " polar " type owing to the position of the high-latitude prominence zone which should then be in about latitude 70° in both hemispheres.

In my last paper I gave some brief references to the intimate association between prominences and the lower corona. That the form of the lower corona is due to prominence activity immediately underneath it is now, I think, generally conceded, and the very common references now to " coronal arches " over prominences appear in connection with nearly every eclipse photograph.

Historical Notes.

In spite of the importance of the knowledge of the changes of the positions in latitude of the zones of prominences during a sunspot cycle, it does not seem generally known that it was Professor Respighi who first drew attention to the fact, that, about sunspot maximum, the prominences reached the polar regions when at other times they were absent from them.

This information will be found in a translation of an extract from Respighi's fifth note on " The Frequency of the Protuberances and their Periodical Variations : and on the Distribution of the Protuberances over the Surface of the Sun and their Periodical Variations." This is given in the Appendix II. to *Solar Physics*, by Sir Norman Lockyer, which was published in the year 1874. The special reference to which attention is wished to be drawn is that given on p. 665 in the above volume and is as follows :—

> " The examination of the curves in the different solar zones through five periods clearly shows that, by the progressive increase of the solar spots, the large protuberances are not only rendered more frequent on the zones of the spots themselves, but also to great distances, even up to the poles, where generally the conditions necessary to their development are wanting."

The periods referred to embody the interval of time from 1869 October to 1872 April and are of about five months each in length. While the numbers of days of observation in those periods are not uniform, they are sufficiently so to bring out quite clearly the facts recorded above.

Summary.

1. The object of this paper is to display on one diagram the periodic changes of several solar phenomena, from 1870 to 1930, to demonstrate the close relationship between prominence action and the forms of the corona.

2. The new data include those for the years 1920 to 1930.

3. These data show that the forms of the corona are dependent on the latitudes of the zones of prominences.

4. Special references are made to the eclipses of 1908, 1918, and 1926 as regards the types of corona recorded.

5. References are made to Ludendorff and Mitchell, who advocate sunspot action, and to Evershed and Bergstrand who favour prominence action as the cause of the coronal types.

6. Forecasts are made as to the forms of the corona up to the eclipse of 1940.

7. Brief reference is included regarding the action of prominences on the form of the lower corona.

8. A historical note draws attention to the first record, by Respighi, of the appearance of prominences at the solar poles about the epoch of sunspot maximum only in the solar cycle.

The Numbers of Stars per Square Degree, Magnitude 11 *to* 14, *in Different Galactic Latitudes.* By P. J. Melotte.

(Communicated by the Astronomer Royal.)

The determination of the photographic magnitudes of stars down to the fourteenth in forty Kapteyn Areas, from plates taken with the 26-inch refractor at the Royal Observatory, Greenwich, now being printed, provides material for a determination of the number of stars per square degree down to this limit.

The magnitudes given in the volume depend on measures made on at least two plates taken on different nights. In the majority of cases three, and even more, plates were obtained. On each plate the polar region was photographed both before and after making the two exposures on the field. In every case the area measured on the plates was $40' \times 40'$, this being the size of the field over which the star images were sensibly round. A comparison of the observed magnitudes of the polar stars with the final magnitudes as given in *Transactions I.A.U.* (vol. ii.) gave the following correction to the 26-inch magnitudes:—

$$+ 0^{m}\cdot04 - 0^{m}\cdot10\ C_{m}.$$

This determination is not a very strong one, as all the fainter stars have large colour indices, and in making the counts given in the accompanying table no correction to the observed magnitudes was applied. Table I. gives the number of stars to each limit of photographic magnitude for each of the forty Kapteyn Areas. These are arranged in three groups according to galactic latitude: 0° to 20°, 20° to 40°, 40° to 90°.

The great variation in star density in the Milky Way itself is evident in the counts included in the first group. The following notes, which refer to certain of these areas, are based on an examination of the Franklin Adams Chart, and a comparison of the Kapteyn Area with the surrounding field $17° \times 17°$ square.

63, 88. Fair samples in regions of great star density.

64. Falls in a very dense region but is distinctly below the average, being at the edge of an area of markedly lower density.

73. A fair sample in a rich region, but falls near a number of "vacant" spaces.

The other areas included in this group are fair samples of the surrounding region.

The areas included in the other two groups are fair samples with the exception of No. 72. The greater part of this field falls in a "vacant" space.

Table II. gives the logarithm of the average number of stars per square degree to magnitude m for each group of galactic latitude, the figures from the table of Seares and van Rhijn being given for comparison.

TABLE I.

Kapteyn Area.		Magnitude.						
No.	Gal. Lat.	11·0.	11·5.	12·0.	12·5.	13·0.	13·5.	14·0.
63	+10°	9	12	24	41	79	133	214
64	− 1	15	35	50	74	116	216	335
73	−12	2	6	7	11	21	33	49
74	+ 1	9	17	24	39	56	90	132
75	+14	7	9	15	21	45	72	125
86	+15	8	12	20	32	45	89	146
87	+ 1	13	20	28	36	51	80	115
88	−12	14	27	35	58	88	143	226
51	+22	6	10	10	17	29	42	69
52	+34	3	8	14	17	21	27	43
71	−34	6	9	12	23	28	37	51
72	−24	3	3	3	5	8	13	19
76	+27	8	8	11	15	22	35	52
77	+39	3	6	11	19	21	35	44
85	+27	4	7	14	21	34	56	76
89	−23	7	9	17	24	39	65	92
90	−34	3	6	18	28	39	52	68
13	+53	2	3	6	9	10	17	..
30	+60	3	3	7	9	10	12	24
31	+68	5	8	12	13	13	15	21
32	+72	4	4	6	9	10	15	20
53	+47	2	5	6	10	14	26	29
54	+60	5	9	11	12	16	21	26
55	+74	4	5	8	10	12	14	22
56	+80	2	3	4	6	10	16	23
57	+85	3	3	3	5	9	13	18
58	+73	4	6	8	12	16	20	23
59	+59	2	3	8	9	9	13	..
60	+48	5	5	9	12	17	23	33
68	−47	4	6	7	10	18	33	48
69	−47	3	5	10	13	15	20	29
70	−42	2	4	6	11	13	18	25
78	+54	6	6	9	10	13	20	38
79	+67	1	5	5	7	10	18	22
80	+76	2	2	3	4	9	13	19
81	+76	3	3	4	5	9	18	28
82	+66	2	3	7	8	13	20	26
83	+54	4	6	8	12	16	24	34
84	+40	3	4	8	8	11	23	38
91	−43	4	6	8	11	20	27	42

Table II.

m.	0°–20°.		20°–40°.		40°–90°.	
	Greenwich.	S. and v. R.	Greenwich.	S. and v. R.	Greenwich.	S. and v. R.
11·0	1·34	1·22	1·03	0·96	0·87	0·76
11·5	1·59	1·43	1·22	1·17	1·02	0·95
12·0	1·76	1·63	1·44	1·36	1·20	1·14
12·5	1·94	1·84	1·63	1·56	1·32	1·32
13·0	2·15	2·04	1·78	1·75	1·46	1·50
13·5	2·38	2·24	1·96	1·93	1·63	1·67
14·0	2·58	2·44	2·11	2·11	1·80	1·83

In the galactic zone there is a constant difference of 0·12 in log N_m between Greenwich and S. and v. R. For the fainter stars there is a progressive diminution in the value of log N_m as the pole is approached, when compared with the table of Seares and van Rhijn.

The Discovery of Pluto. By W. H. Pickering.

In connection with Dr. Crommelin's interesting paper under this heading which appeared in *M.N.*, **91**, 380, 1931, I wish to point out two misapprehensions. He kindly sent me a proof-sheet, but it reached me too late to permit me to indicate them to him before his paper would be published. Since the proof is not paged, I can only locate the first correction by saying that it occurs in a paragraph a little past the middle of the paper, and beginning with the sentence "Pickering's method was a semi-graphical one" (p. 382). In the eighth line of this paragraph, for the words "was in conjunction with" we should substitute the words "showed a maximum perturbation due to." The method of prediction used does not depend in any way on the conjunction, and no attempt was made to predict it. Indeed it cannot be determined with any accuracy, and the gist of the whole problem was to find some method to avoid its use. The date of the maximum perturbation of Neptune from the orbit which it would have followed had it not been perturbed by Pluto was on the other hand very readily determined by the graphical process employed,* and the date gave the longitude of Neptune at that time, from which the longitude of Pluto was computed in a very simple manner. The data employed were taken from the observations published annually at Greenwich and less frequently at Washington, aided by correspondence with the Superintendent of the Observatory in the latter city. These observations have lately been further confirmed by Kostinsky at Pulkova.†

* *Harvard Annals* (1908), **61**, 113. Later and briefer, *Popular Astronomy* (1928), **36**, 143.

† *Bulletin de l'Observatoire Central à Poulkovo*, **12**, 1, 1930.

The second misapprehension in Dr. Crommelin's paper is identical in nature with the first one. In the first line of the following paragraph, for " conjunction of the unknown with Uranus in 1859 " read " maximum perturbation of Uranus due to the unknown planet in 1865·5.' The last sentence of that paragraph should be omitted.

Comparing Lowell's investigation with that made at Harvard, it will be seen that the two methods employed were unlike in nearly every respect. Lowell adopted the old analytical method invented by Adams and Leverrier, and made use exclusively of the perturbations of the planet *Uranus*. The writer employed a graphical method, invented by himself, which was much simpler and quicker, and which gave a much more accurate position of the unknown planet, as we shall see later. He employed the planet *Neptune* primarily, and only used Uranus, and temporarily Saturn, in a very secondary way, in order to deduce a provisional orbit. He considered that the important thing was to find the planet, and that the orbit was of no use or consequence other than to correct the heliocentric longitude from the date of maximum perturbation to the date of observation. The method involved then, primarily, the determination of the date of maximum perturbation of Neptune by graphical methods ; secondarily, the finding numerically the corresponding longitude of the unknown planet on that date ; and thirdly, the correction of that longitude by means of the provisional orbit to the date of observation. It was intended and expected that this interval would be so brief that even an inaccurate orbit of the unknown planet would serve every purpose. The writer was therefore satisfied to adopt the perturbations of Uranus for this approximate result, although he ultimately rejected those of Saturn. Had he then known how small the perturbations of Uranus due to Pluto really were, he would have rejected them also, and used Bode's law, as was done by the computers of Neptune. Omitting the latter planet from our consideration, since it fails to conform to the law, Bode's law would give Pluto a mean distance of 38·8 compared with the true value 39·5, a much more accurate result than any computer obtained.

Lowell could not use Neptune with the analytical method of computation which he adopted, because its observed orbit was only half completed, in consequence of which its elements were uncertain to the full extent of the perturbations. The reason why I used Neptune in my work, on the other hand, was because I saw that its maximum perturbation was obvious and considerable, * 3″·9, while that of Uranus was indefinite, uncertain, and small. Therefore the empirical method of finding the planet was devised. But in order to compute an orbit it was necessary to fall back on Uranus. Neptune has shown no perturbation hitherto due to any planet beyond Pluto, but should such a perturbation appear in the near future, we should have to wait until the end of the present century, when the orbit of Neptune would be nearly completed, before we could safely apply the analytical method of computation to it. It is possible, therefore, that the empirical method may again be used to advantage, instead of waiting so long, and it is

* *Popular Astronomy* (1928), **36**, 151 ; (1931), **39**, 2.

indeed possible that, through Lowell, the analytical method has seen its last application.

We have spoken of the perturbation of Uranus by Pluto as being rather small. It is not difficult now to compute approximately what its maximum amount was, due to the last conjunction of the two planets in 1850. Let P be the maximum perturbation of Uranus due to Pluto. Let $3''{\cdot}9$ be the maximum perturbation which Pluto produced on Neptune after passing its recent minimum distance 18 units from that planet in 1892.* Let 30 be its minimum distance from Uranus in 1853.* Let $30{\cdot}0$ and $19{\cdot}9$ be the radii vectores of Neptune and Uranus, and $1^\circ{\cdot}30$ and $3^\circ{\cdot}03$ the angular speeds with which these planets passed Pluto at conjunction. Then we have

$$P = 3''{\cdot}9(18 \times 30{\cdot}0 \times 1^\circ{\cdot}30)^2/(30 \times 19{\cdot}9 \times 3^\circ{\cdot}03)^2 = 0''{\cdot}58.$$

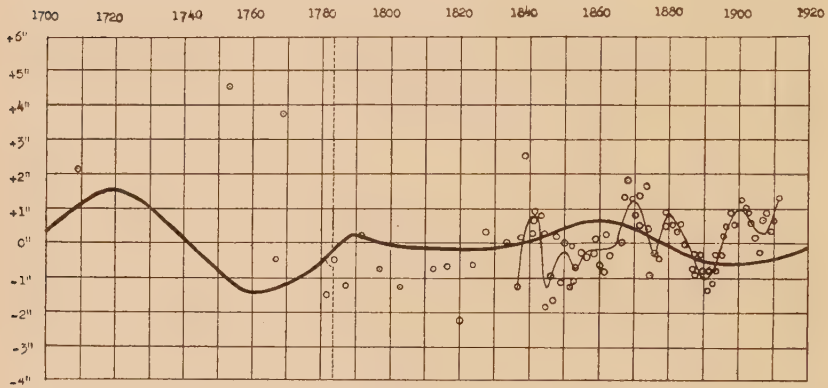

FIG. 1.—Gaillaud's residuals according to Lowell.

Let us now compare this result with the observations from which Lowell computed the position and orbit of the planet. Fig. 1 is taken directly from plate ii. of his Memoir on Planet X, now called Pluto. The abscissæ give the dates, and the ordinates the observed deviations of Uranus in heliocentric longitude from the path which it would have followed had it been perturbed only by the then known planets. A + ordinate indicates that Uranus was ahead of its computed position. The deviations are those given by Gaillot, based wholly or mainly on the Paris observations, and are rather markedly irregular. To Lowell's drawing I have added the heavy curved line which shows in a general way what the perturbations of Uranus due to Pluto really were. In other words, the observations should all coincide with this line, if they were without error. The conjunction of the two planets occurred in 1850, although they were nearest in 1853. Given the date of conjunction only, it is impossible to compute the date of maximum perturbation, but it is very certain that the latter cannot have occurred more than ten years later than the conjunction, or about 45° of longitude. A more

* *Lick Observatory Bulletin* (1930), **427**, 40.

probable value would perhaps be 20° or 30°, but I have given the maximum value in the drawing in order that the curve may agree as closely as possible with the observations. To calculate the accurate perturbations from the present mass of data would be a rather pretty problem, and perhaps someone will undertake it, but the present curve cannot in general be far out of the way, and will perhaps indicate among other things why Lowell computed a mass for Pluto that was ten times too great. I have also added to Lowell's figure the vertical dotted line. The only astronomer, so far as I am aware, who has ever carefully gone over Lowell's work and checked it up, is Professor E. W. Brown. The others who have commented upon it, like Russell, have merely noted in general that Lowell's orbit was of approximately the correct size, that his longitude of perihelion was also fairly correct, and his resulting position of the planet not very far out of the way. Its position was about 6° in error when it was discovered, and about 7° in error on the date for which its position was computed in 1914–5. Professor Brown says: "It then appears that the actual values which he (Lowell) obtains for the distance, mass, and eccentricity substantially depend on three groups of observations made before 1783, having large probable errors." * These three groups are indicated by the five little circles to the left of the dotted line.

At its latest conjunction with Pluto, Uranus passed at a distance of 30 units from that planet, or at about the mean distance of Neptune from the Earth. Owing to the great eccentricity of the orbit of Pluto, however, at its previous conjunction Uranus passed it at a distance of only 17 units. Hence the perturbations were then much larger. This may perhaps in part explain Lowell's erroneous value of the mass which counts so much against his work with many astronomers. Fortunately the size of his orbit was not affected by this fact, and considering the scattered nature of the observations made prior to 1783, on which the size, as Professor Brown says, "substantially depends," I think that we may agree that Lowell was surprisingly successful. Indeed, no other computer of unknown planetary orbits has ever done so well. His computation of the semi-major axis was only a little over 8 per cent. in error. Leverrier's was over 20 per cent., Adams's nearly 24, in spite of the very numerous observations on which their work was based. All three gave too large a result. My third orbit was 24 per cent. too small, and was indeed the only one out of twelve predicted orbits described by Crommelin that was smaller than the true one. Lowell's longitude of perihelion was also excellent, only 18° or 10 per cent. out of the way; but this extraordinary accuracy, depending as it does largely on the later observations of Uranus, when the deviations were small, may well in part have been due to accident. Lowell's great achievement, however, was undoubtedly his successful computation of the size of Pluto's orbit, taken in connection with the scattered observations from which it was derived, for it is the size of his orbit together with the longitude of the perihelion that makes it coincide so closely with the true orbit of Pluto, as computed first by Crommelin. It is this fact which filled Russell

* *Proceedings of the National Academy of Sciences* (1930), **16**, 365.

with amazement, and caused him in his paper on the orbit of Pluto to declare that it was "incredible" that the close agreement of the two orbits could be due to accident.* Nevertheless Professor Brown remarks: "The conclusion indicated here that such a determination of the elements of a hypothetical planet has almost no weight is supported by the numerical data which follow." † And again: "We must therefore regard the fact that it (the planet) was found near the predicted place as purely accidental.‡

Referring now to my own results, I wish again to point out that my empirical method gives only a position for the unknown planet on the date of maximum perturbation of the known one. In order to obtain an elliptical orbit, one must either similarly employ two perturbations of another known planet, as I did in the present case, or use one perturbation combined with Bode's law. In any case the search should be made as soon as possible after the date of maximum perturbation has been recognised, so that the orbit shall not have had time to develop any large error. It may be further pointed out in this place that whether the analytical or the empirical method of search is employed, the only things of importance to determine are the location of the planet and its brightness. The elements of the orbit can be far better determined after the planet has been found, and their approximately successful prediction is merely of interest as a personal triumph of the computer.

In closing I may remark that I am not claiming to share the principal credit for the discovery of Pluto with my late friend Dr. Lowell. One or the other of us should have the primary credit for it alone. We both computed the position and the brightness of the little planet for definite dates, by different methods. As to which method gave the more reliable result, based on the deviations recorded of Uranus and Neptune, I leave others to decide. My first computation was based on supposed perturbations of Uranus, and placed planet *O*, as I then called it, in heliocentric longitude 105°·8 in 1900·0. Neptune was then in longitude 85°·7, and I noticed and stated that Neptune was gradually being dragged forward in its orbit ahead of its proper place.§ It seemed to me, however, that it would be impossible to make use of this perturbation until after the year 1924. In point of fact I was able to use it in 1919, and computed the position of Pluto for the epoch 1920·0.‖ Four plates were taken at this time at the Mt. Wilson Observatory in the computed place, but the planet was not found upon them. Ten years later some plates were taken at the Lowell Observatory on which Pluto was found, according to Crommelin in a longitude differing 5°·8 from Lowell's computation. The magnitude that the latter attributed to the planet was 12 to 13. As soon as the orbit of Pluto was accurately determined, the Mt. Wilson plates were re-examined, and the planet was found upon them, differing from the computed place by 1°·15 in longitude, and of the computed magnitude 15.¶ Neither Lowell nor I ever

* *Scientific American* (1930), 446.
† *Proceedings of the National Academy of Sciences* (1930), **16**, 368.
‡ *Ibid.*, **16**, 371. § *Harvard Annals* (1908), **61** 155. ‖ *Ibid.* (1919), **82**, 59.
¶ *Harvard Annals* (1908), **61**, 155; *Lick Observatory Bulletin* (1930), **427**, 35; *Popular Astronomy* (1931), **39**, 3.

saw any of these plates, and we therefore were not responsible in any way for finding the planet upon them, except in so far as Lowell left money to have the search made. I made no such gift. If it was a mere accident, as some claim, that his position was so near to the actual one, then it was certainly a very fortunate accident for the finding of the planet. Had it not been for his bequest, it is quite probable that the planet would not have been found for many years, and he should receive therefore full credit for this very important part in the discovery.

The mass of Pluto based on my determination of the extent of its perturbation of Neptune is 0·71.* Nicholson and Mayall have recently published a value of 2/3 of that of the Earth,† thus indirectly supporting my value of the extent of the perturbation of Neptune. As to the very interesting question of the density of Pluto, that can only be determined by visual measures of its diameter made with the largest modern telescopes. The only such measure so far made, as far as I am aware, is by M. Baldet with the 32-inch refractor at Meudon. He states that its diameter cannot exceed 4000 miles. This would give a density to Pluto of at least 31, or one and a half times that of platinum. Although Pluto is situated 22° to the north of the equator, yet the seeing is so bad at all northern observatories during the winter months when Pluto can be well seen, that it is probable that the best measures of its diameter can be made in South Africa, where the seeing is good during our northern winter.

Private Observatory,
Mandeville, Jamaica, B.W.I.:
1931 *March* 31.

A New Method of Second Difference Integration on the Brunsviga-Dupla Machine. By E. Chappell.

(*Communicated by L. M. Milne-Thomson.*)

The writer, after extensive use of the method of second difference integration published in *M.N.*, **88**, 447 (1928 March), came to the conclusion that although the method there described was certainly workable, it could not be considered convenient if many-figure second differences had to be set in the machine by means of the thumb wheels of the first product register. This means of setting is so slow that an improved method was looked for, with the result that, at the cost of one extra turn and clearance per value, the second differences could be set on the levers instead of in the first product register, with a noticeable gain in convenience and speed.

It happens that the work on which the writer is engaged involves the use of the function itself to find the second difference. Using the

* *Popular Astronomy* (1931), **39**, 6.
† *Publications of the Astronomical Society of the Pacific* (1931), **43**, 75.

old method, which required the differences in advance, it was necessary to estimate mentally the first few figures of the next value of the function and after a simple calculation to set the second difference on the machine, obtain the next value of the function, and see whether the provisional value assumed was in agreement with the value as obtained ; if not, a simple revision put matters right. By the method about to be described, second differences are not required in advance, but in arrear. This had better be described by the following scheme :—

Function.	Δ′.	Δ″.	
A			*Old Method.*—Having obtained *B*, *Y* (a " forward " second difference) must be set on the machine, so that the next turn adds *M* to *B*, giving *C*, and *M* to *Y*, giving *N*.
B	*L*		
C	*M*	*X*	
D	*N*	*Y*	

New Method.—Having obtained *B*, *L* (the previous first difference) is added and subsequently *X* (the " central " second difference).

Four cases arise according to whether the signs of the first and second differences are like or unlike that of the function. After three preliminary processes which differ, the procedure is exactly the same in all four cases. No complementary numbers have to be set, and after the start all turning is forwards.

Case.	*f*.	Signs. Δ′.	Δ″.	Set First Value of Function in R_2.	Set First Value of Δ′ on Levers.	Direction of First Turn.
I.	+	+	+	White	Direct	Forwards
	−	−	−			
II.	+	+	−	Red	Direct	Backwards
	−	−	+			
III.	+	−	+	White	Direct	Backwards
	−	+	−			
IV.	+	−	−	Red	Direct	Forwards
	−	+	+			

Cycle :

1. Transfer R_1 to levers.
2. Turn.
3. Set first value of Δ″ on levers.
4. Turn, and read R_2.
1. Transfer R_1 to levers.

It would, of course, be possible to interchange R_1 and R_2, but there are two disadvantages that would follow :

1. The result register could not be locked against accidental clearing.

2. The transfer lever would have to be moved to its second position each time.

Note on Mr. Chappell's Method of Second Difference Integration.
By L. J. Comrie, M.A., Ph.D.

The method of second difference integration described by Mr. Chappell in the foregoing paper is by no means new ; it has been in use in the *Nautical Almanac* Office for more than two years, particularly with a Burroughs Class 11 machine, which prints all its results—argument, function, first and second differences. This machine has two adding mechanisms, known technically as the " crossfooter " and the " register." The function is developed in the register and the first difference (or its complement) is carried by the crossfooter. After printing a function value the direct value of the central second difference is set on the keyboard and added to or subtracted from the first difference in the crossfooter. The new first difference is then added from the crossfooter to the register, but remains intact in the crossfooter for further development. These operations have resulted in the printing of the second and first differences, and finally the argument and function are printed. The machine develops values of a function from its second difference at the rate of 500 to 600 per hour.

When my paper in *M.N.*, **88,** 447, 1928, was written the Nova-Brunsviga Model IVA had not appeared. It contains 18 figures in the product register, with a device (the use of which is optional) to prevent the clearing of the left-hand 9 figures in this register when the clearing lever is operated. This feature enables this machine (which is much less expensive than the Brunsviga-Dupla) to be used very conveniently for second difference integration, with the convenience of lever setting of central second differences. The function is developed in the left of the product register, the " partial clearing " lever being in operation. With the carriage in the extreme right-hand position (*i.e.* No. 10) the first value of the function is set on the lever, and turned into the product register. The direct value of the first difference is now set on the levers and also turned in, forward or backward according to its sign relative to the function. The carriage is then moved to position 1, and the first difference turned in forward, *i.e.* into the right of the product register. The direct value of the second difference is set on the levers and turned in, forward or backward according to its sign relative to the first difference. The new first difference is then transferred by the automatic transfer to the levers, the carriage moved as far as possible to the right, and by a turn of the handle the next value of the function is developed in the left of the product register. It will be noticed that no complements are used. This process is slightly longer than the Dupla process, because of the carriage movements, but as neither machine prints, and as the greater part of the working time is spent in copying results and in setting, the difference is unimportant.